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North Central Forest Experiment Station

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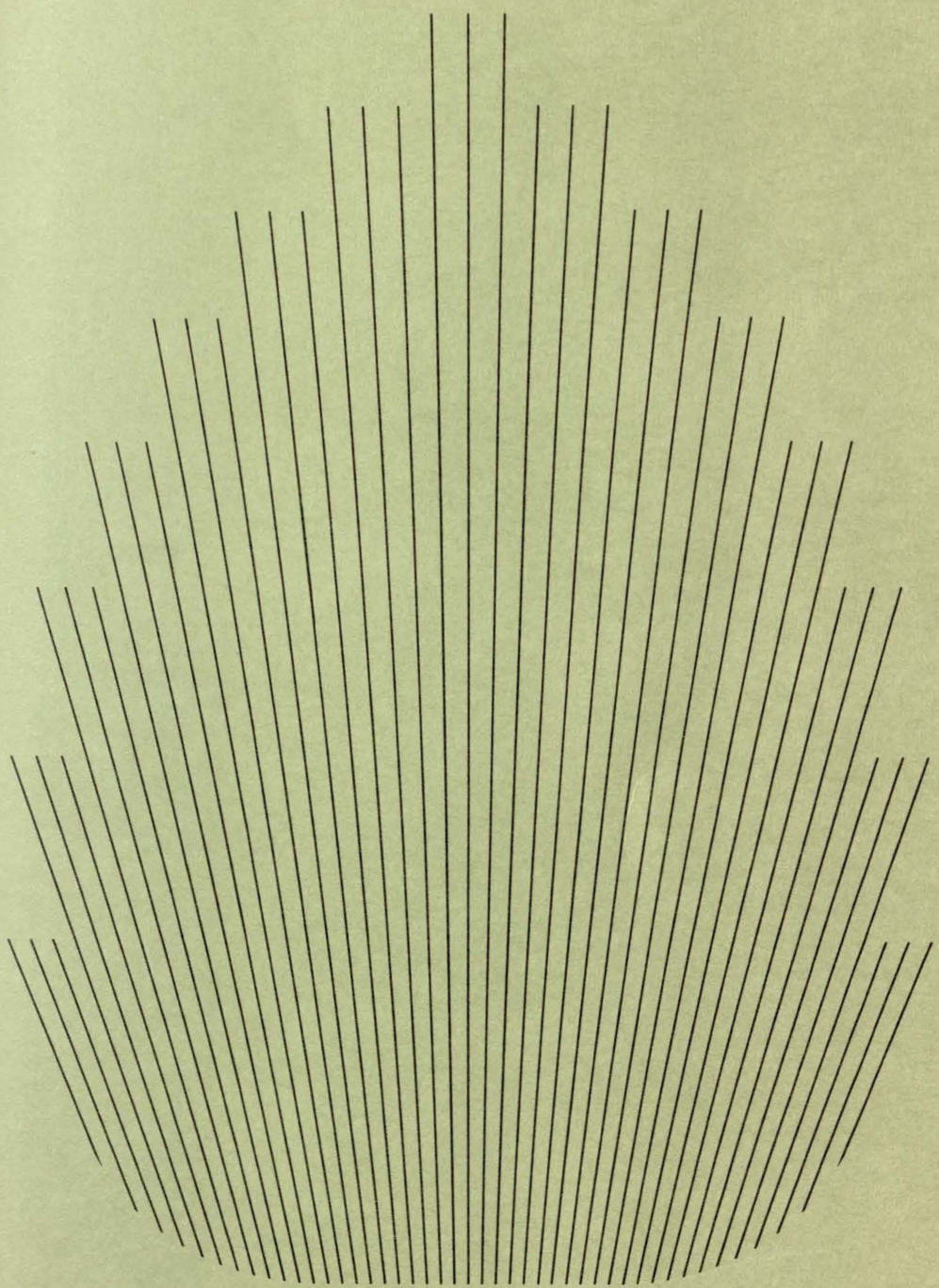
Forestry Research Evaluation: Current Progress, Future Directions



North Central Forest Experiment Station
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FORESTRY RESEARCH EVALUATION: CURRENT PROGRESS, FUTURE DIRECTIONS

Proceedings of the Forestry Research Evaluation Workshop
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PREFACE

This workshop had its origins in the National Planning Group (NPG) meeting in Kansas City, Missouri, in April of 1984. The forestry research leaders at that meeting agreed that the time was proper for a review of efforts made so far in forestry research evaluation. The task of organizing such a review naturally fell to the Regional Planning Group in the North Central States (RPG-2), because of the location of three key researcher groups in St. Paul, Minnesota. These are the North Central Forest Experiment Station's Research Work Unit "Methods for Evaluating Forestry Research," and the University of Minnesota's College of Forestry and Department of Agriculture and Applied Economics. The latter researchers in particular have been recognized as national leaders in the evaluation of agricultural research.

Held under the auspices of NC-RPG-2, the workshop was co-chaired by Gary Rolfe of the University of Illinois and Robert Hann of the North Central Forest Experiment Station. Hosting the workshop was shared by the College of Forestry at the University of Minnesota and the North Central Forest Experiment Station.

An effort was made to involve all researchers engaged in forestry research evaluation, with the purpose of assessing the work completed and in progress, and to consider where future efforts should be directed. To keep the review process to a minimum at the workshop itself, summaries of studies were provided by the researchers and sent to participants in advance. Thus, everyone would be familiar with the latest completed studies and also with those in progress. This left more time at the workshop to discuss the uses of research evaluations, the reasons why they are requested by policymakers and administrators, and the characteristics that make evaluations most useful. Future needs for evaluations were also discussed.

As in any gathering of interested parties, the contacts made and discussions pursued during breaks and after the daily sessions had ended were an important aspect. Because of the newness of this research effort, it was useful for researchers to find out what others were doing, what problems and opportunities they had encountered, and which studies and methods they hoped to pursue next. From this standpoint, the workshop was a success.

Current research in this area developed with impetus from several sources. First, the Deputy Chief for Research in the Washington Office of the Forest Service expressed a need for research evaluation. Second, administrators in the Cooperative State Research Service expressed a similar need. Third, researchers at the North Central Forest Experiment Station expressed an interest in pursuing this line of inquiry. Much of the credit for establishing the new Research Work Unit at North Central must go to Allen Lundgren, who laid the groundwork through his interest in the subject and his ability to engage other researchers at several universities in forestry research evaluation. The development of this area of research has been strongly aided by university researchers—those at the University of Minnesota and Duke University, in particular—and by their training of graduate students in this new area of endeavor.

Chris Risbrudt

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INTRODUCTION AND WELCOME

Gary Rolfe,
Department of Forestry,
University of Illinois,
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I am pleased to welcome each of you to this workshop. I believe that the topic of research evaluation is one that is important today, but tomorrow it's going to be critical, both to funding of research programs that we're all interested in and to our ability to meet the research needs in the forestry arena. It's quite timely that we have a conference on research evaluation particularly related to forestry research.

Some similar research evaluation studies have been done in agricultural research and I think that they have traditionally paid off. Research evaluation studies in relation to forestry have paid some dividends as well. So, the short of it is that I think this is a good conference to be at. We can make some real progress today and tomorrow and I believe it's time that we considered the topic of research evaluation and make some plans for being ahead of the game.

Often, in quite a few research areas, we find ourselves in the situation of immediately responding to an emergency or to a need for research or for planning. It's time we get ahead of the game, so that we anticipate some information needs over the long run and do some planning that will allow us to effectively respond. Research evaluation is one tool we have to help us in that process, and it's a very important tool.

Research evaluation is especially important because it can help us to be better researchers. In the next few years that is going to be critical, because, as all of you know, we are seeing an erosion of formula funding or block funding for different research areas, including forestry. I think that trend is going to continue and we're going to see an increase in the competitive grant approach to research funding. To be competitive as scientists we are going to have to be on top of the game. That means we will have to know what we're doing, why we're doing it, what objective we're going to reach, how soon we can get to that objective—and we will have to do that all in a cost-efficient manner. So research evaluation can benefit us in these areas. It can help us do a better job of productive research, it can help us to be better research planners, and it can help us reach our objectives in providing good, hard forestry research results in a much more cost-effective way. I think that's the real question that we're going to get down to in the end. How much can you do for how little of the resource? That's always the case, it has always been the case, and it's going to be much more important in the future. So, I don't think I can say too much about the importance of research evaluation.

Getting beyond the individual researcher, research evaluation studies are important at the State and Federal levels. The people who are working in that end of the system have to be able to demonstrate definitive data which support what they're doing in terms of productivity, in terms of the results being useful, and in terms of cost effectiveness. One way they can obtain the information they need to support these arguments is through good studies, especially good evaluations of research programs. In considering funding both at the individual researcher level and at the State and Federal level, research evaluation studies can genuinely pay off.

In addition to the funding aspect, I think that at the National and State level research evaluation studies can help us identify gaps in our research data base, isolate research areas which need more attention, and support and justify moving our research program in appropriate directions.

There are several reasons for research evaluation studies, and this afternoon as we discuss these studies I'm confident we all will agree that this is a good time to consider research evaluation and how it can lead to more effective research planning. This particular conference came about as a result of the RPG-2 Planning Group meeting in Kansas City last year. Unfortunately, I was unable to attend, but as the RPG-2 co-chairman with Bob Hann, it was our responsibility to organize this gathering to look at research evaluation. It worked out quite well, because I really didn't have to do anything at all except sign my name on a few letters. The North Central Forest Experiment Station and the University of Minnesota actually did the work in getting you all here and making this conference happen. I think the fact that we're here and can make some progress over the next day or so is very important.

The objectives of this workshop are twofold, at least, and I'm sure that over the day and a half that

we're going to be here we'll identify some new areas that we need to discuss as well. The first objective is to discuss the current uses of research evaluation studies and to project the sorts of research evaluation needs that we may see in the future. What types of things do we need to do in the long run to really be ahead in our planning process? That's a very important objective so we'll start off taking a look at what we're doing now. Hopefully, we'll have some good discussion with everybody involved as to where we need to go with research evaluation studies. The second objective of the workshop is equally important: To bring together scientists who are currently doing research evaluation studies and allow them to share with administrators and other scientists the purpose and objectives of their program and the results that they have obtained to date. Looking at the study summaries, there is quite a bit of common ground, but we can all learn by thinking about different approaches and different techniques.

Certainly research evaluation has some new tools that can be used over the coming years. The computer tools that are available will help to more efficiently do these types of studies. We have a significant opportunity over the next day and a half to talk about this important subject. We have the people here who can help to define and plan a long-term program of research evaluation, which we very definitely need in the forestry research area.

RESEARCH EVALUATION NEEDS AND USES

RESEARCH EVALUATION AND POLICYMAKING

John Fedkiw,
Associate Director,
Office of Budget and Program Analysis,
Office of the Secretary,
U.S. Department of Agriculture,
Washington, DC

It is rewarding to be a part of this forestry research evaluation workshop because so many evaluators are participating and much evaluation is going on, something I have supported for many years. There is a serious interest at both the managerial level and the research analyst level, and I note from the advance materials that the forestry research project we are here to review is beginning to have useful and notable first fruits. Chris did not give me any specific instructions or topic to address.

He leaves me feeling like a first lieutenant at point with no discernible targets and no clear orders. From that standpoint, some background views on the need for this evaluation effort seem to be an appropriate place to begin. After that, I will have comments on how we got into program evaluation, the usefulness of evaluation results, new information priorities, and the value and use of the evaluation results in research planning and decisionmaking. I will approach my subject with a broad view to give you some feel for the environment in which this type of information is used and how it is applied.

THE NEED FOR RESEARCH EVALUATION

As a start on the question of the need for research evaluation, I would like to take you back to the beginning of beginnings—the story of the Garden of Eden and of Adam and Eve, our first progenitors. Eden is a place without any demand problems, without any supply problems, no technology needs, and no new knowledge needs. It didn't last. It was not long before our progenitors violated certain basic principles and they were evicted, with a divine direction: "Henceforth, you shall work by the sweat of your brow." Note the great symbolism in the choice of words—by the sweat of your brow and not our backs. It doesn't take much whimsy or imagination to take that divine command as the beginning of research and a blessing of goodness on all research and all researchers. In that context, research can be seen as truly the first profession. Claims for the first profession among the denizens of the "red-light district" are truly apocryphal. After all, with only Adam and Eve, there could be no factual basis for the latter claim. It takes three to make a triangle.

So, almost from the very beginning, we had to find out for ourselves how to provide for our demands, produce our supplies, and develop all the technology to serve 4 billion progeny today and, in the not-too-distant future, 2 billion more people. That little story reflects the goodness of research and researchers, and the need for evaluations. What and how much is research doing for the progeny? There is divine goodness in what we do, but the value of what we do is judged by the progeny among its other needs. I felt the need to address the question of goodness because when we first addressed program evaluation for forestry research in a formal way at USDA in 1966, it upset a lot of people. I remember one researcher in particular. He is now one of the strongest supporters of program evaluation. In 1966, however, he saw it differently. During an extended dialogue about the desirability of program evaluation, he suddenly stopped and said, "John Fedkiw, you don't appreciate the goodness of research." And there has been more than one researcher who has repeated that view over the years.

Let me bring the story down to earth by paraphrasing the Deputy Secretary of Agriculture's views from a dialogue on the forestry research budget last Thursday. The point had been made that forestry research had declined 17 percent in the last few years and that agricultural research had grown significantly. The Deputy Secretary responded with an analogy, but his point was, "we believe in the goodness of forestry research but when I speak about research allocation, the people ask me, what are we getting for the investment?"

The Assistant Secretary for Natural Resources and the Environment has been making tradeoffs among forestry programs at USDA for several years.

They unambiguously indicate a perception of low productivity for research at the margin compared with other alternatives for increasing forest productivity, timber supplies, and other resource supplies. There is no other way to interpret it. His views are not explicit about where those margins are within research, so he leaves it to the Deputy Chief for Research in the Forest Service to make those decisions.

One area, however, has been highlighted, and that is hardwood management research in general. Inventories have increased 50 percent since 1952, because we have been harvesting only half of the growth. These inventories will continue to grow for 20 years or more even as we increase the harvest of that hardwood inventory. The question is, how much has hardwood management research contributed to that abundance? How critical was the technology in the past to achieving the current abundance? How critical is it likely to be for future supplies? I don't have clear answers for these questions. One is left to make a practical judgment on the basis of the availability and outlook for hardwood supplies and prices. There is no question about the goodness of the research in this area, but goodness does not provide sufficient guidance for a practical answer to the research allocation question.

One can cite similar critical questions in other segments of forestry research, as well as some specific real benefits and contributions. It isn't all one-sided. The bottom line is that we have done some good, but we have little quantitative measure of how much good. There also appears to be room for improvement. It's a mixed bag. It's confounded by the fact that we do not have overall measures of forest management productivity itself, the contribution that research makes to it, or the criticalness of research to future productivity. Conceptually, there are answers to these questions, but we don't have any relative measures.

There is no general policy question about the intrinsic goodness of forestry research or about the need for a reasonable level of scientific capability in this area. The practical issue of need is concerned with resource allocation decisions at the margin.

To cap this general note on the need for research program evaluation, let me cite an agricultural viewpoint from the first paragraph of the preface to the Proceedings of the 1969 Symposium on Resource Allocation in Agriculture Research. That was about 15 years ago.

> "In any area of man's endeavors, it is good for him occasionally to take stock of where he is at present, and what he has accomplished. With this new perspective, he then can more effectively design and carry out his future pursuits, whatever they may be. This volume is the result of such stock taking; in this case, the focus has been on efforts to evaluate the productivity of agricultural research and attempts to plan and direct such research in and by public research organizations."

THE BEGINNING OF FORESTRY RESEARCH PROGRAM EVALUATION

In my experience, forestry research program evaluation as a formal activity began in the mid-60's with Planning, Programming, and Budgeting (PPB). This was the Johnson Administration's effort to allocate resources more efficiently to nondefense programs, governmentwide. The key tools were *ex post* evaluation of program effectiveness in providing net benefits—that is, benefits to national welfare—and *ex ante* analysis of the benefits and costs of new initiatives.

The first application in forestry was *ex ante* analysis of the benefits and costs of southern pine genetic improvement research. The results were endorsed by the Department and sent to the Office of Management and Budget (OMB) as a credible analysis and projection. They were used explicitly in justifying the tree improvement segment of the research budget and they were highly successful in doing so. I remember the Deputy Chief coming to me after the final budget marks were out about a month later and saying that the research budget outcome that year was a historical first. He was not sure about all the reasons for it, but the *ex ante* analysis was a part of it. It was the first time the rate of increase for research exceeded the national forest allocation.

Shortly after that, the Forest Service assessed the costs and benefits of a selected technology in each major research area from utilization down to forest protection, including insects, disease, and fire. This, I believe, was *ex post* evaluation. The results showed a very high rate of return for forest products and utilization research, and relatively low marginal or submarginal rates—depending upon what you consider the capital cost at that time—for both fire research and insect and disease research. That pattern suggested that research efforts which improved or developed new products or reduced their cost of production were highly productive. This is confirmed in one evaluation project we are reviewing today—the work Hans Gregersen has completed on lumber and lumber products research. It shows a 36 percent rate of return on the marginal research investment.

Research benefits that are long deferred appear to have a relatively low social value. That has some implications for short rotations, improving species

substitutability, and removing the dimensional requirements for large trees. Some of the most successful research has worked in those directions. Most of the work that has achieved those kind of results, however, has been done in utilization areas. We have learned how to use smaller trees; we have learned how to use hardwoods in place of softwoods; and we have learned how to use thinning to shorten rotations and increase tree size. Your current short-rotation research evaluation project covers one initiative that addressed all three purposes simultaneously. That research involved substitution of a relatively unused species (sycamore), a short rotation of 3 to 8 years, substitution of a hardwood species for softwoods, and some reduction of the dimensional requirement for pulpwood.

There was a long lull in forestry research evaluation after that study of comparative productivity, about 5 or 6 years. Then, in the middle 1970's, a General Accounting Office review of the 10 most productive projects identified by Forest Service research and a similar study by the Department's Office of Audit revealed some serious shortfalls in the transfer of research results, their operational feasibility, and their utility. These findings led to specific efforts to increase the effectiveness of technology transfer and to seek more effective feedback from potential users at all stages of research planning, execution, and transfer of results. These evaluation efforts were followed by a broad Forest Service initiative to develop criteria for defining allocation priorities for future research as a part of 1980 RPA. It did not deal with the current base program, but recognized it would turn over in the future and would be part of the priorities. There also were some quick and dirty *ex post* evaluations for selected results of completed research. The latter were not and could not be generalized. Some of the evaluation work was questionable.

Policy decisions on the 1980 RPA research program responded favorably to the structured analytical commitment and effort. The policy officials felt the Forest Service was making a genuine effort to allocate resources in a way that would maximize expected benefits. A 5 percent rate of funding increase was proposed. The emphasis was on increasing national forest productivity and on a number of emerging problems such as acid rain and desertification. The Department officials strongly withstood efforts of OMB to reduce the proposed rate of growth. It was challenged because it was higher than the rate of growth proposed for the Defense Department in that year. Shortly after the 1980 RPA decision was finalized the North Central Forest Experiment Station's research evaluation project was established.

USEFULNESS OF RESEARCH EVALUATION RESULTS

My comments to this point have already provided some views on the usefulness of research evaluation. Here, I'm going to offer a more comprehensive and structured assessment. I will cover *ex post* and *ex ante* evaluations separately.

Ex post Evaluation

Ex post evaluations are most useful and valuable where they address large program aggregates or subaggregates. Individual project evaluations have little utility or value in decisionmaking. Individual completed projects are historic events with fixed costs, and unlikely to be repeated again. Such evaluation results cannot be used as sufficient measures of the general productivity of the research programs they represent. Results often vary widely, from deeply negative to highly positive. However, we seldom will evaluate the deeply negative. The results may be additive if they are truly representative and if they are based on consistent assumptions, methodology, and benefit measures and unit coefficients. I would like to illustrate the importance of aggregate research evaluations or the evaluation of large subaggregates from experience in agricultural research.

The most important single bit of *ex post* evaluation information about agricultural research is the understanding of how much research has contributed to the overall productivity of agriculture, total food and fiber supplies, farm and consumer price levels, and general national welfare. I can illustrate this best by quoting a statement Secretary Block made in 1981. It reflects both the understanding and knowledge that policy people have of agricultural research productivity in the *ex post* sense and the value of that information:

> "During the 1960's, American agriculture experienced a tremendous advancement in technology and research. These factors allowed agriculture to produce more abundantly and far more efficiently than at any time in history. In fact, so much more was produced that it caused a price depressing oversupply...
>
> "Since 1950, our agriculture increased its output by nearly 70 percent while increasing input by just 4 percent.
>
> "Even more significant is the fact that today agriculture is the largest net contributor to our balance of trade with a surplus reaching toward 30 billion dollars."

That is a clear, strong perception of the value of agricultural research. In 1965, during the early days of PPB, we made a similar assessment. We found we were producing our agricultural output in 1965 with the same real cost of production that we had used to produce the output in 1950. Both domestic consumption and exports were substantially higher in 1965 than in 1950.

Not all improvement in agricultural productivity can be credited to research. There is systematic analysis, however, that suggests that about a third of the growth in this productivity can be attributed to public investments in research and extension. There is much documentation of agricultural research productivity and its benefits in the aggregate and for major subaggregates. It is widely understood and applied. There is also confidence that agricultural research can deliver the desired results with an acceptable probability of success and a relatively small increase of funding. That's an understanding shared by the U.S. Department of Agriculture's Director of the Budget, and he expresses it as his understanding of the perception of the Department's policy officials.

Nevertheless, that has not been a guarantee for unqualified public investment. In the earlier part of the 1960's, Federal agricultural production research was being continuously recommended for reduction or no increase in budget reviews. Its actual funding by Congress reflected those recommendations through the mid-1970's. Production research was seen as contributing to the growth of surplus commodities and record federal costs to reduce that surplus.

Agricultural productivity improvement was observed to slow in the late 1960's. It was also observed to slow in the 1970's, after a sharp increase in 1971. These patterns may or may not have been a response to the slowdown in research funding. Nevertheless, these perceptions, combined with the sudden and rapid expansion of export demands after 1972—mainly, grain exports to Russia—raised new concerns about the productivity of agricultural research. Several studies on the effectiveness of agricultural research and on new priorities for an expected long-term rising trend in world food demands and U.S. exports were commissioned, primarily by Congress. A great deal of congressional attention was focused on means to improve both agricultural productivity and related research productivity. This resulted in identifying new research priorities, emphasis on basic research, and an increase in Federal funding which has continued through this Administration.

The important point of these comments on agricultural research is the understanding of how important agricultural research is to the productivity of agriculture. That is basically an *ex post* evaluation understanding. There is a widespread understanding of the linkage between agricultural productivity, supplies, and prices and agricultural research productivity.

There are good measures of agricultural productivity and they are updated annually. But in forestry we don't even have a good measure of productivity as an aggregate measure for the country or for a major subregion. That may be an information priority you will want to think about. It seems to be a fundamental need in evaluating how forestry research relates to total forest productivity.

Measures of research productivity in agriculture are less regular and generally less adequate for year-to-year assessments. Nevertheless, there is enormous public and policy sensitivity to that known linkage with the productivity of agriculture. Congressman George Brown has been concerned about long-term agricultural productivity and commodity supply as they relate to world food demands and welfare, as well as U.S. welfare. Other members of Congress have concerns that are more local and related to farm income in their States and districts, and to short-term results and applied technology. For example, during 1966-1976, when funding for Federal research was flat, Federal grants to States for agricultural research continued to rise, and State funding for State agricultural experiment stations actually accelerated. The States did respond, including their representatives to Congress, to funding of State-oriented agricultural research.

Thus, the agricultural productivity research linkage exists not only at the national level, but extends to the State level wherever agriculture is a major economic activity and agricultural productivity is an important determinant of State comparative advantage and economic welfare. However, there is a difference in Federal and State concerns and it is epitomized by an anecdote about the USDA's Deputy Secretary for Agriculture. When he received a report recently that a land grant university in a leading milk-producing State had found a way to increase milk production per cow significantly with a simple injection, he responded that it was "UDDERLY terrifying research!" Given the concerns for surplus milk and cheese and the huge costs of USDA purchase programs, that assessment conveys more than a sense of humor. Obviously, milk cow productivity does not have high priority for Federally funded research.

The concerns are not always generalized across the board; exceptions are seen. Congressman Brown has been particularly strong in trying to get the

USDA to be more specific and more complete in *ex ante* analysis of research targets to increase productivity.

In contrast to agriculture, in forestry there is very little clearly known or understood in quantitative terms about the short-term or long-term linkages between forest productivity, supplies, prices, and the productivity of forestry research. The problem is more complicated than a lack of measurement of research productivity. We do not have any general measures of forest productivity trends for the total sector or its major subaggregates to serve as a focal point for concerns about research productivity. More than that, we do not have any measure of the productivity of controllable forest management inputs and how they affect total productivity, and how critical forestry research is to the productivity of those management inputs.

These last observations provide a broad perspective on where we are with forestry research program evaluation in the *ex post* sense, and offer a broad target for where this evaluation research should be headed. Despite the foregoing views and general assessment of forest research effectiveness, there is a general understanding among policy officials that forestry research does contribute to short-term and long-term forestry productivity, even though it is not well quantified. They are aware of case examples, and in general tend to extend the agricultural research experience with respect to productivity impacts to the forestry research sector. There is a commitment among policy officials to maintain an effective level of scientific capability in forestry research. Policy officials also respond incrementally to new problems and opportunities of national significance. One that was of particular interest to the Deputy Secretary in 1984 is, "Where are we at with acid rain; do we need to do more in identifying and understanding that problem?"

Ex ante Research Evaluation

In contrast to *ex post* evaluation, *ex ante* research evaluation is most useful and valuable when applied to specific technological problems or technology targets. It is probably the strongest option for critical justification of research projects to develop new technology for production purposes. It probably has little value for research issues where the major concern is uncertainty and the major purpose of research is to define the dimensions of a problem or identify viable options for amelioration of a problem. Acid rain, of course, is a case of this type. An example where *ex ante* evaluation was usefully applied was the 1960's evaluation of the potential net benefits of southern pine genetics research which I cited earlier.

CONCLUDING REMARKS

I have come to the end of my remarks and will close with the common saying among informed staff people concerning the evaluation and review of annual program proposals and budgets. The productivity of a program is and will be evaluated in each budget year, empirically or judgmentally, as well as comparatively, whether or not a systematic quantitative evaluation is available. When quantitative evaluation data are available, they will be taken seriously, as well as critically.

Insiders from OMB have said more than once that the improvements over the years in evaluation or analysis for program justifications have made their task much more difficult to perform. My response has been that they are mistaken: it's much easier to do. But it is more difficult to reduce justifiable initiatives supported with sound analyses, even in the face of budget ceilings.

The general philosophy of research is that systematic, verifiable, quantitative analysis is superior to empirical observation and judgmental evaluation. That type of analysis also is more certain in *ex post* measurement than in *ex ante* forecasts.

The support of research program evaluation, both *ex post* and *ex ante*, is a proper, legitimate function of program administration and management. It rests primarily with the managers who assess the effectiveness of program aggregates and subaggregates and determine priorities for resource allocation. Evaluation results can be a powerful tool for research managers because such results provide a basis for disinterested judgment in allocating public revenues to enhance national welfare or State welfare. It can be particularly helpful in the face of the "thousand hands" on the inside and the "thousand hands" on the outside, who are annually trying forcefully to help managers divide the pie. But it will take courage to use that evaluation tool openly and straight forwardly. A large volume of credible evaluation results both in *ex post* and *ex ante* mode is probably the best source of such disinterested fortitude in resource allocation.

DISCUSSION I

DENVER BURNS

I would like to ask a question concerning timeliness of evaluation. Agricultural production evaluations are now many years old. The ones that have been done in forestry will age quickly. How relevant is that timeliness to the usefulness of evaluations for policy?

JOHN FEDKIW

I think the relationship and linkage between agricultural research and agricultural productivity has been established, quantified, and updated regularly. The quality of the evaluations has also improved each time. The last comprehensive evaluation of agricultural research productivity, including extension, that I'm familiar with was done by Bob Evenson about 1980, at Yale University; he was formerly with the University of Minnesota. His finding was that about 50 percent of the agricultural productivity could be attributed to investments in research, extension, and the education level of the farmer.

LYNN MAISH

I think timeliness depends on which type of evaluation is made. John's main point is to get this understanding of the linkage between productivity and research. That's the sort of background information that is useful in policy decisions. Once you achieve some broad understanding of that basic knowledge for policymaking, timeliness becomes more important in smaller decisions.

DENVER BURNS

I guess my real interest here is at what point did it pervade the policymakers' consciousness that agricultural research was a good thing? For agriculture, did that start and stop with hybrid corn, or has it been the recent spate of articles? There are many good cases in forestry, and yet they don't seem to have any lasting effect. Part of that problem is that we have not expressed research effectiveness very well.

JOHN FEDKIW

The very beginnings of the U.S. Department of Agriculture in 1862 were concerned with improving productivity by distribution of better seeds. That, in a sence, was an extension program. But there was a research program that also began then. It was a Federal research effort. Later on in the 19th century the public decided there ought to be research at the State level also. That led to the formation of the land grant research program and system. From about the middle of the 19th century up to about 1910 quite a bit of research was done. U.S. productivity in agriculture did not appear to be increasing and it became the source of great concern in the early years of this century. There were discussions among the agricultural scientists and the policy officials about what to do about it. One of the focal points of that dialogue became, "We have got to get some way to take this technology and get it to the farmer." There was a Federal initiative for that, and there was also a State initiative. It worked out that the State initiative would put a technician in each county to work with each farmer. One-to-one contact that could transfer that technology became the extension legislation, the Smith-Lever Extension Act. A contract was written up that the States will administer that law, carry out and execute the extension, while the Federal government will only administer the legislation in a broad, general way. Once that was put into place, it had great impacts on the transfer of agricultural technology and productivity improvement. There were some really great assessments of those impacts from 1915 to 1955, or 1960. There are high marks for that extension and agricultural research linkage in technology transfer. Agricultural research wasn't going any place until extension got linked into it. Today that linkage may not be quite as effective as it used to be, nor perceived as having the same value. Part of it results from the evaluation work that has been done. Perceptions change. For example, in the 1960's and early 1970's, there wasn't any great support for agricultural production research because of the crop surpluses. That has now changed because of the outlook for world demands and the need to build up supplies and to remain competitive on a price basis in world markets.

DENVER BURNS

Perceptions do change, for we're just now seeing forestry move from land-based towards technology-based management. The last of the forest lands are being allocated to various uses. That parallels what occurred in agriculture when lands were no longer available.

JOHN FEDKIW

I don't think we really understand what we're trying to do in the aggregate with respect to forest resources, productivity, and production in hardwoods versus softwoods. I think we've got an abundance of things working in different directions. In my view, the work that has been done in wood products research by industry, the Forest Service, and universities in some cases is equal in productivity to agricultural research. The story is probably equally spectacular in terms of keeping the costs of production for wood products down and supplies relatively abundant. Although real increases in stumpage prices may have obscured some of the benefits in the case of softwood products, we have just as spectacular a story to tell there as we have in agriculture.

When you add to production or reduce losses, you are saving the last timber that will be harvested, so to speak, from the current inventory. This was recognized when we were doing mountain pine beetle control. There we spent quite a few million dollars and finally decided it was not cost effective. We were trying to save stands that wouldn't be harvested in 80 years anyway. There was an effort in western white pine blister rust control that did not work. On the other hand, we've got southern pine plywood technology, which was developed at the Forest Products Laboratory. We've got waferboard. We've got hardwood pulping. We've got press-dried linerboard from hardwoods coming as a substitute for softwoods. Softwoods are scarce. Hardwoods are abundant. There's quite a bit going on in this forestry world, in terms of efforts to increase productivity, save resources, and things like that. Some very gross evaluation work can be quite revealing in this area, because it has never been put together in a systematic, analytic way.

ALLEN LUNDGREN

That raises a question, John. We're just getting started in forestry research evaluation, according to my perception of the field. We are mainly doing some individual case studies. How useful is that kind of information in building a perception of forestry research productivity among policymakers compared with comprehensive studies—aggregates or subaggregates of forestry research which may be beyond us at this point?

JOHN FEDKIW

I think the piece that was done on lumber and wood products is a good start on evaluation of subaggregates of forestry research. But you have plywood, you have hardwood substitution for softwood pulpwood, and a whole set of projects to extend the existing wood supply by making low quality timber and species substitute for very high quality timber and species through changes in conversion and utilization methods and technology. Sum these changes up and see what technology has done for us in this area with respect to wood supplies. What would our wood supplies have been if we hadn't had the new technology? We would be building houses out of brick, sand and glass, or something like that if we hadn't had that technology. I think it has been an enormously great benefit. I don't think we can show the same thing in other areas of forestry research. It is not that they are unimportant and not that we shouldn't continue to do work in management research, protection research, and other areas, but we ought to assess the expected net benefits when we expand or allocate research funds beyond the level for maintaining scientific research capabilities.

DAVID LEWIS

John, in the area of natural resource policies, does part of the process involve looking at new technologies, as opposed to other policy instruments for achieving whatever stated goals you might have?

JOHN FEDKIW

For the 1980 RPA, we had a list of 14 issues that were prepared by the Forest Service and presented to policy officials. There were two issues added by policy officials. One was a request for an assessment and projection of the supply impacts that can be expected from emerging technology. They were not looking for a certain solution. They wanted to understand the supply potential of the visible, emerging technology. The Forest Service economists made such an assessment working with the researchers, and a lot of the results were judgmental. Like Vern says, you have to talk to scientists to get some really good information, in making ex ante *projections. It is my recollection that the economists and scientists judged that the emerging technology would reduce the demand for supplement timber supplies by 8 million board feet by 2000, over and above the 1980 RPA assessment estimates. That is a large increase in supply in 20 years. But no one in the Forest Service came forward and said, "We need money to do this." We didn't ask for more, although it may be one of the reasons we got the 5 percent increase. I do not recall that the priorities for utilization research were very high.*

DAVID LEWIS

So are you saying that we dropped the ball ourselves in a sense?

JOHN FEDKIW

I don't think the issue was pressed very hard, and I don't understand why. I've gotten involved in examining the credibility of some of this technology. We're looking at plantations today and sorting out what the results have been since genetically improved stock has become widely dispersed. There is a project underway to evaluate the on-the-ground benefits of the tree improvement work as it is beginning to show up in young plantations. This tells us that what we assumed in the 1960's for southern pine genetic improvement may have been high, reflecting what can be expected on managed research plots as opposed to outplantings with less oversight and care. So the actual productivity improvement may not be as high as projected in the ex ante *evaluation. Nevertheless, the actual benefits achieved are probably worth substantially more than the research costs. I would like to see a similar evaluation of the genetic research at North Central. I think that's a piece of work that is ready for assessment.*

ROBERT BUCKMAN

I don't dispute the notion that the biggest gains in forestry have come from utilization research, but I also have the feeling that there is a very strong neglect of what the biological contributions have been. The utilization gains tend to be easily identifiable, easily described...something tangible and concrete that you can hold in your hand...structural flakeboard, press-dried paper, things of that sort. Whereas a lot of the gains in biology have been incremental and lots of people have made small contributions, but in the aggregate the contributions are enormous. The resurgence of the forest resources in the South in my estimation is the Cinderella story of forest conservation in the world, and yet it is a story that is not well told at all. And it began with education and some fairly simple technologies, such as fire protection, getting hogs and livestock off the range, etc. Today we are approaching the time when the South will supply half the forest products of the country. It is a story in biology. It started out very simply. But with thousands of small increments in the nurseries, in planting practices, in tending, in insect and disease control and fire control, we have made enormous contributions. The same is true right here in the Lake States. I think we need to give more attention to the biological side of technology.

JOHN FEDKIW

I agree that there have been some significant things done on the biological side of forestry. Part of the work that impressed me was led by Mann in the Southern Station on tree seedlings, planting stock improvement, and grading. I was rather impressed with the kind of things that were coming out, because they led to better survival rates and lower production costs and also better growth performance because of a better grade of seedlings. That was a very important piece of research that made a big difference in the productivity of and the interest in tree planting. I am aware of some of these things, and perhaps in some species they have been important. Southern pine would be one. Where fire is concerned, probably the biggest contribution came from the institutional and behavioral changes that took place in the courts and among farmers and other people.

ROBERT BUCKMAN

But you made the point earlier that Evenson makes that you can't separate education from science.

JOHN FEDKIW

He sorted out the increments of research, extension, and the educational level of the farmer. I believe he came to the conclusion that while extension was a smaller investment it was as productive as the research itself, using multiple regression methods. Returns to extension were highly dependent on the level of research investment, and returns to research were modestly influenced by extension according to that analysis.

ROBERT BUCKMAN

The question that has been around before, for me and for my predecessors, for as long as I have known is, "Why are you doing this when the industry should be doing it?" This goes way back before this Administration. We had a very formal inquiry on that issue from OMB a couple of years ago. We made out a set of criteria that should guide Federal investments in research. One of them had to do with backstopping regulatory programs. There were about six criteria. We did a 100 percent assessment of our portfolio of research studies in the Forest Service to identify any possible industry-type studies, and then we followed up with a series of telephone interviews on top of that. We've been so sensitive to that issue, that I really would like to think that we positioned ourselves, by and large, in areas where the industry itself would not do the research. About 80 percent of our studies are in the nonproducts field—they are in biology, protection, inventory analysis, etc. And about 20 or 22 percent are in the products and engineering field. In the products and engineering field we try to deal with things that have a consumer interest or that backstop Federal land management programs, have a regulatory context, or a long-term basic, or a major speculative or risky, aspect. If that research is successful, if

those kinds of studies, the basic research or the risky long-term research targets are successful, they ultimately lead you to the point where it is useful. The thing that I objected to most in the OMB study was that OMB staffers tended to treat this research segment, which ought to be a continuum, as a discontinuity or to introduce a discontinuity into it. When the research approaches utility then you drop it and somebody else will pick it up. I don't happen to agree with that. This ought to be a continuous process. Those of you who have been to the Forest Products Laboratory in Madison, WI, have seen one, two, or three emerging technologies like press-dried paper, which could be a very big contribution in the near term. When the utility of press-drying became evident, we invited industry to look over our shoulder and talk about it. That is a model of the way we ought to work with the private side. The industry said we need to know certain things before we will make the first investment. We built a small pilot plant machine. We built a high-speed simulator that would mimic production speeds, pressures, and temperatures. About the time we finished with that the industry said we had carried this far enough. We essentially dropped that as a main line of work and moved off into other technology targets like recycled fibers, and things of that sort. Avoidance of duplication of industry research is an issue that we have been extremely sensitive to. I would like to think that relatively little of our work is in the product development area. Now there is one exception; in some of the areas of economic downturn, such as the Appalachian area, we are doing some work that gets fairly close to application. I have a suspicion that if we didn't do it nobody else would. Things like upgrading hardwoods so that they can be better used in furniture stock and things of that sort.

THE AGRICULTURAL RESEARCH EVALUATION EXPERIENCE

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The reason that we're interested in research evaluation is because we want to allocate research resources. I will discuss some of the principles that are involved in thinking about the allocation of research resources. These are the issues upon which we want to bring analysis and judgment to bear in our research evaluation studies.

Whenever we allocate a research dollar, we're always either explicitly or implicitly answering two questions.

The **first** of these questions is: What are the possibilities of advancing knowledge or technology if resources are allocated to a particular commodity, problem, discipline, or geographic region? The interesting thing about this question is that to get a good answer, one must ask those who are at the leading edge of their discipline or profession. In any given area, there are relatively few researchers who can imagine what can be done beyond what is being done now. In answering this question, the intuitive insights of economists, planners, and administrators are not very much help. We have to extract that knowledge from the best scientists in the various fields.

The **second** question is: What will be the value to society of the new knowledge and the new technology if the research is successful? Dr. Richard Bradfield of Cornell, who was one of the world's great soil scientists, once commented, "There are thousands of interesting questions—and some of them are important." It seems to me this captures what we're concerned about. Whether we employ formal analysis or intuitive judgment when we allocate research dollars, part of the question we must answer is, "What's the research worth to society?"

I argued above that the intuitive insights of economists, planners, and administrators are not very useful in telling us what can be done. The intuitive insight of scientists, or of administrators who used to be scientists, is not very good at telling us what things are worth. We must attempt to answer both the question of what can be done and how valuable it is to do it. We can approach the problem at many levels of sophistication. But if we keep in mind that these are the questions we're trying to answer, even in areas that we have to approach intuitively, the chances are that we will be able to make better decisions.

Let me note three approaches that are commonly used. The model frequently used in making research resource allocation decisions might be called the parity, or congruence, model. This model is implicit in the question, "Did I get my share?" Just for a definition, if the percentage of dollars spent on, for example, maize research, is equal to the percentage of the value of commodity production contributed by maize, the ratio is 1.0. Nobody really believes that the ratio should be 1.0. But in discussions about the allocation of research resources, the congruence model is frequently implicit. One can calculate congruence ratios by commodity—for example, maize. One can do it by geographic area. Some of you may know of a study published by the University of Wisconsin that concludes that the Midwest doesn't get its share of the Federal research dollar. One can also calculate a ratio for factors of production—for inputs. That's one we don't do very often. If we did I think we would put a lot more resources into the human factor. One can examine research resource allocations by levels of economic activity—by the resources that go into production, and by the resources that go into post-harvest activities. One can also examine allocation by discipline. When I hear department heads arguing about budget cuts or budget increases, I frequently find them asking, "How did soils do relative to agronomy, or to agricultural economics?"

Even though nobody really believes that the congruence model is a very useful guide to research resource allocation, it does provide useful information. If I were a research administrator, and I only had one staff person to help me in research planning, I would want him to give me the congruence ratios

by commodity, geographic area, factor of production, levels of production, and discipline. Then I would apply my intuition and ask whether the ratios make sense. When I look at the major agricultural commodities, I find that we're spending about $1.50 in research per thousand dollars of commodity value for most of the major field crops. A little bit less than that for maize, a little bit more than that for soybeans. We're spending something like $34 in research per thousand dollars for sheep and wool. The ratio doesn't need to be 1.0. But I doubt if anybody is ready to argue that the existing ratios make sense. The ratios for the minor horticultural crops, and even some of the major ones, tend to be between 8 and 15 dollars. There are some major anomalies. Some of them can be explained. But we don't rationalize across commodities very well.

Once one gets beyond the parity model, then what kind of more formal methods do we use? We're all familiar with the peer review system in which you send a project to a group of people who are supposed to know something about the project. The peer review panel scores or ranks the project. The scoring model system works best where the objective is clear. If you've already decided you're going to work on the physiology of fuelwood trees, then you can evaluate fuelwood tree projects with scoring methods. You can ask the people who have a capacity to think and know about physiology and fuel trees to evaluate and rank the projects. The ranking will give you some idea of which people can do the work and are likely to succeed.

But when one attempts to rank across the whole agricultural and/or forest area, the scoring model breaks down. I remember being involved in a scoring effort when I worked in the Phillipines. When it was all through, somebody looked back and said that what we had done was pure nonsense. After everybody put their scores down, it turned out that we had placed four times as much value on saving a peso by import substitution as we had on gaining a peso from exports. Any scoring system has an implicit set of shadow prices.

Another set of methods we see being used, particularly in agriculture, is what I would call experimental approaches. The experimental approaches have the great advantage that they can be highly decentralized. Yield-constraint studies attempt to assess the sources of yield differences between farm produce and experimental yields. An attempt is made to allocate the difference into those components that can be addressed through research, those components that have to be addressed through resource investments, those components that are affected by economic factors. Farming systems research is another way to generate this information. The advantage is that the results, usually based on interdisciplinary efforts, can feed right back into the research system—i.e., if you have a system that permits decentralized research decisions, then that knowledge can feed right back into research decisions. You don't have to convince the people involved in the research of what is important.

Within agriculture other experimental approaches have been used. For example, the selection index approach has been used to determine what a bull is worth. A relatively new area is plant growth models. It has the potential of getting down to just what constraints on plant growth are worth changing.

If we can evolve systems that are sufficiently decentralized in deciding what research can be done and what research is worth doing, then these experimental approaches are highly valuable. We need to develop systems for feeding that knowledge back up through the system without too much distortion. One of the great problems of feeding that kind of knowledge back up through the system is that at each level there is a tendency for information to degrade.

A major challenge is how to link the project selection process, or the process by which the scientific and technical judgments are brought to bear, with macro models to define sector-level research resource allocation decisions.

In any research resource allocation system, one of the things that should be kept in mind is the size of the decision one is making relative to the size of the effort that goes into the analysis for the research decision, always keeping in mind that the research is designed to make better decisions. The resources going into the research to make better decisions could also be used to do the research itself. What is the value of improving the decision?

We see some things that are rather disturbing in this area. For example, the California Legal Action Project, in its suit against the California Agricultural Experiment Station for wasting the State's money by spending it on development of tomato harvesters, is asking for a social impact statement on each research project. If that happens, we can expect some combination of (1) people lying about expected impact because the typical research project is too small to be able to carry out a thorough impact analysis, and (2) research projects getting a lot larger. That might be desirable, because I see a lot of research projects in the Minnesota Agricultural Experiment Station that are too small.

DISCUSSION II

ROBERT BUCKMAN

In forestry, Vern, I have the perception that there may be a wider set of alternatives for some agencies than there is in agriculture. In your opening remarks you said that one of the reasons for evaluation is to choose among the alternative uses of research funds. It seems to me in forestry when we have research closely linked to some fairly major action programs, that the choices could be even larger than that. Should the funds go into research or should they go into operational activities on the land itself? West Virginia Pulp and Paper Company was confronted with a problem not so long ago about whether they should purchase an additional 60,000 acres of land. They anticipated that their wood needs would increase, and the question facing them was whether they should purchase the additional land or increase productivity per acre by 10 percent. They chose to go with the 10 percent route—that is, to increase productivity 10 percent. They made a significant investment in a laboratory at Summerville, South Carolina, and felt it was more effective and cheaper to go that way. The point I'm making here is that it strikes me that the choices may be even more numerous in forestry.

VERNON RUTTAN

I agree. Any organization has to decide how much to invest in the future, in a sense, and how much to invest in the resources to expand production with existing knowledge and technology.

DAVID LEWIS

Vern, you raised an interesting issue which had to do with the allocation of resources to the evaluation process as opposed to doing the research program being evaluated. I'm curious if we have any experience that gives us either rules of thumb or some perspective on just what that allocation is?

VERNON RUTTAN

One rule of thumb is that you ought to have a good reason for departing from parity. If you knew nothing about the values of society, or about the possibility of research outcomes, you would then use the parity model. As you know more you can depart from the parity model.

CHRIS RISBRUDT

Bob, do you see the parity model being applied in forestry?

ROBERT BUCKMAN

Yes. The parity argument is normally used as I see it to favorably influence your case. We sometimes call this the index method. You always try to make invidious comparisons between somebody else's program and yours.

DENVER BURNS

That's essentially a historical trend approach which was developed in the criteria papers as being applied in RPA.

VERNON RUTTAN

It's amazing how many of these research systems one can go into and the director or associate director can't really give you those numbers. Really, all it takes is one person to spend a little time on it and you can have those values, either relative to sales or to value added.

CHRIS RISBRUDT

That's one of the problems we have in forestry. We can value so little of our research or so few of the many outputs of forestry. We have trouble with values.

VERNON RUTTAN

Again, it's a question of how precise you want to get. Even in agriculture, we haven't had published value added by commodities until fairly recently. You can approach that if you have some budget studies, or cost of production studies, if you know something about the inputs, about the production, and about the post harvest areas, and approximate value added, in the production area. Even doing it on a sales basis, or in forestry on a stumpage basis, these are the kinds of numbers I would want to know.

ALLEN LUNDGREN

It seems to me this parity model is some kind of formula funding for research. Isn't the McIntire-Stennis program a formula funding for research

based upon the extent of land area and other factors? Isn't that another kind of parity?

VERNON RUTTAN

I use the term parity deliberately here because most people think parity is a bad idea. So when I say you are using the parity model, it makes you stop and think a bit. I'm sure you've all been at meetings where somebody gets up to the blackboard and says, let's put the objectives on the board for each of these programs, and put up a big matrix, and let everybody rank them and see where we go. I think that can be useful as long as the objectives are narrow enough. But if you ask your research assistant to go out and calculate the implicit shadow prices of those rankings, you are likely to get some funny answers.

I think it is a device that people use for arguing a case. It avoids a lot of sophistication. There's something intuitive about it. So when Wisconsin—I think it was the Associate Director of the Experiment Station—comes out with this bulletin that says in essence, "The USDA is not spending enough in the Midwest. We account for 66 percent of the farm output of the country and are only spending 40 percent of the research dollars," it implies that something's wrong.

DAVID LEWIS

I think that's the question. Is it wrong? The real problem is the parity system. For organizations that supposedly thrive on change and are devoted to change, to base their allocation resources on history, strikes me as being ridiculous.

VERNON RUTTAN

It does me, too. The reason I want those numbers is so that I can depart from them. I want to know what the numbers are. Then I want to use them in developing the logic to show why we should change. One of the reasons we're low in agricultural research expenditures in the Midwest is that we grow crops that are grown on millions and millions of acres. Also, we grow a crop called maize, in which the private sector is a major performer of research. You spread those research dollars over many, many more acres than you do on potatoes, for example. But still I think the ratios are a little bit high.

ROBERT BUCKMAN

One of the parity numbers that gives me more pause than any other is that the Federal R&D dollars going to agriculture, and I think this includes forestry, now amount to about 2 percent of the total Federal R&D budget. And yet, if I add the numbers correctly, agriculture and forestry generate something approaching 25 percent of the economic activity in the country. I'm not sure about those numbers, but they provide the general idea. Agriculture, and in some respects forestry, enjoy a comparative advantage among the nations of the world. It seems to me that that kind of a parity relationship does tell us something about where the country might be investing. What's flawed about that reasoning?

VERNON RUTTAN

It's partially flawed in the sense of how it's broken down. In other words, take that 25 percent; one is counting not only the inputs of the agricultural production sector and the forest production sector, but taking the inputs used to produce them, such as farm machinery and fertilizer. So we need to add the research that's done on farm machinery. When we figure that for agriculture, we take all those wooden boxes that you forestry guys produce and we add that to get up to that 25 percent. Also, we add in the paper bags and the labor that is done in restaurants. It's important to make sure that whatever the definition is on the value side, we match that with the inputs on the research side. We shouldn't compare it just to the production research.

DAVID BENGSTON

You mentioned the University of California trial, and the concern about the social impacts of research. Do you think that we're missing the boat in forestry research evaluation, at least in the work that's been done previously, by not looking at those social impacts?

VERNON RUTTAN

I think society has a right to expect us to look at those social impacts. Any State agriculture experiment station or Federal research agency ought to have an ongoing program of technology assessment, in which the economic and social implications of potential research results are discussed with the clientele as part of public affairs extension programs. I regard the State of California as very vulnerable. I think the remedy being sought would be disastrous. The State is very vulnerable because for years they had only one man working on farm labor markets. When the mechanization process was coming along there should have been a dialogue within the State. What kind of safety nets do we need? For years, there was absolutely no protection, just as there was no Social Security for farmers until the mid-50's; there was no protection for farm workers. In that kind of situation, you

know you are going to get a political backlash. I think it makes sense to have generalized protection so you don't have to decide that a person who got a new job two weeks after he lost his job as a tomato picker is not going to be compensated, whereas somebody else who couldn't get a job for two years is going to be. If you have specific protection systems they're going to really tie you up, whether it is a tomato harvester or a Toyota that puts you out of work. But if you have a generalized protection system, then some of the political backlash can be avoided. My feeling is if we can make sure the public affairs extension program is taking these issues to the State, then we're going to have a better environment for making those decisions.

RICHARD SKOK

Based on your experience with agriculture, do you have any observation on the degree of applicability and receptiveness of research evaluation and analysis at either the State or Federal level? Is there a difference in these two?

VERNON RUTTAN

The Federal system is under new Congressional pressures to rationalize the decisionmaking process. I see that coming along in the State now. For example, in the last legislative session there was a proposal to conduct a study to advise the States of how they should go about making research funding decisions and evaluating the effects of the research. That process is underway right now.

REFLECTIONS ON FORESTRY RESEARCH EVALUATION

Richard Skok,
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As a manager of a research program I am looking for solutions that don't create new problems. In the mid-70's, I served as liaison for the Association of State College and University Forestry Research Organizations (ASCUFRO) to IR-6, which is an interregional committee of the ag experiment stations dealing with research evaluation in agriculture. Bobby Eddleman chaired that committee and there were several ag researchers from each of the regions involved. It was interesting to observe the approaches they were using, the issues they were dealing with, and the enthusiasm they generated not only among themselves, but with ag experiment station directors and with some of the Congressional staff who had access to OMB people.

It was about that time that we were doing the regional and national forestry research planning conferences with which Bob Buckman was so much involved. It was also at the time I had become dean at Minnesota, and John Ohman was North Central Forest Experiment Station Director. It was decided he would be a good one to lead the Federal coordination of those conferences. Someone was needed to represent the schools and, since I was next door, it seemed a logical selection. As we made the rounds on these conferences, I drew on this experience with IR-6. John and I talked about some other possible directions we ought to be working, besides what had become sort of generic planning activities for which forestry became rather well known. The evaluation area was one we thought might be worthwhile, based on what was happening in agriculture. About that time John was transferred into Washington as an Associate Deputy Chief for Research. Out of that came the commitment from the Forest Service to put some money into evaluation research. The Cooperative State Research Service under Jack Sullivan's leadership also made a commitment to financially support a modest effort in this area. The fact that this conference is now being held and that there are many people now involved in research evaluation in forestry bears out the results of these earlier initiatives. The real need that we saw was to build up a cadre of individuals who had the skills, capabilities, and interest to work in this area. While impetus might initially come from one location, we agreed to seek to diffuse it throughout the system. People who were interested and willing to make a commitment in this area of inquiry were the key to accomplishment.

Let me suggest a couple of thoughts concerning this area. One is the question of what is meant by research evaluation. It should be taken in a broader context than simply the economic analysis of research programs. I think evaluation analysis must address the issues of research policy in forestry. At least at the national level for the Forest Service, and at the university sector, we need to deal with emerging directions and present effectiveness of our research programs. There is an interest in better understanding what our research has done, as well as what our research can do to contribute to overall social well-being.

If I bring this thinking back home as a manager of a State-level research organization the "ball game" is quite different. In the pragmatic political environment in which one works there are a variety of factors which dictate the priorities and decisions that you establish for your research programs. Now I am not suggesting that that isn't an important element at the Federal level as well, but I don't think there is, at either the executive or legislative level of State government, the analytical capability in staffing which enables one to use the same kind of effective approach at influencing resource allocation for research.

There is much to be said, and it isn't just intuition, about maintaining a constant flow of communication with those people and groups that we see as our users of research. In the State they are of critical importance to us. One of my observations would be that we haven't done a very good job to date of convincing the practitioners in forestry of the importance of science in their activities. Practitioners often do not make a linkage between what research contributes and their management of the forest resource. I think we are starting to see some gains in

this area, but we have much to accomplish if we compare ourselves to the agricultural experience. Because of my association with the Agricultural Experiment Station, I attend many meetings where the agriculture community is dominant. When you deal with agricultural groups, there is little question in the mind of commercial farm operators about that linkage. It is an assumed relationship when they walk into the meeting. A similar meeting with forest resource management people is a quite different experience. These managers are as much our "farm operators," if you want to use that overall term.

A good share of this shortfall in scientific appreciation might be attributed right back to our education system. The forestry schools often have not done enough in educating future foresters concerning the role science plays in practice. Perhaps it has not been obvious to many of us until recently. Now one can talk about tree improvement and we can talk about many of the scientific contributions that guide present practice. It no longer is just "clinical" observation through practice that we build upon.

Evaluation is an ongoing process and it is not something new, in a sense. Someone always has been making evaluations about research programs. What you are doing through the evaluation studies approach over the last 5 or 6 years is to provide a more formal structure and objective approach. This is desirable in my view and it is something done at least partially out of necessity.

I would echo what both Vern Ruttan and John Fedkiw have said today, that you have to be cognizant of evaluation for whom and for what purposes. From the view of someone in a management mode, I would say we are looking for information that can prove useful to us as decisionmakers and also to us as individuals who try to influence other decisionmakers. In that context, your research evaluation absolutely needs to have credibility. It has to be in the hands of people in the scientific community who are not on the staffs of managers but respected in their own right as researchers. This seems to have been important to the success of the agricultural research evaluation work. The kinds of people that must lead the evaluation studies are people who have a high degree of credibility in their own research community.

For us there is always a danger in commissioning these studies. After all, we may come up with the "wrong" answers. That's a fact we face. Probably a few "wrong" answers would be useful to the credibility of the whole system. The tendency is to pick out winners early on, and obviously not all research is "winning" research. One of the troubling things about research evaluation is assuming that all research has to be winning research. If it is, then it says to me that we are probably not betting on the full range of things that we ought to be when we make our research resource allocations. One of the useful things about the wood utilization evaluation study discussed today is that it looked at several scenarios and included dollars spent on research over a much broader range than just the research that turned out the particular products that proved to be beneficial. It assigned the benefits over a much broader cost basis than traditionally has been done.

The main goal of research evaluation is to improve our research performance because it should improve our ability to manage research. From a practical point of view, we hope research evaluation also will provide justification of new and past programs of research. Work done by the group assembled here today substantiates our belief that this is a fruitful area of inquiry, relevant to the needs of our time, and important to research management and policy. The beginning has been productive yet there remains much to be done.

DISCUSSION III

JOHN FEDKIW

There are many constraints in the whole system of land management planning that have never been researched, and we know generally managers are going to be conservative and safe. Every profession is like that, and they set standards higher than they ultimately need to be. One place where research can increase the supply of timber from the national forests is by identifying resource and environmental management standards that can be set at lower levels. In some cases, they may have to be at higher levels. In any event that is a critical area in the eyes of the present policymakers, all the way up to the top of the Administration, because they're anxious for more timber productivity, and have been for the last 20 years.

One problem that has been observed in the land management process is that constraints are inconsistent between regions. For example, Region 6 has a 1,000 acres, and Region 5 has 500 acres, say, for spotted owls. These constraints impact the rate of harvesting timber, a primary concern of policymakers. Research is needed to provide valid information for spotted owl management, and valid information about other constraints. This is genuine research. Unfortunately, I don't think many scientists see it that way. And that's one of the problems that troubles us. We all say one of the problems in this whole evaluation area is that technical people and evaluation and policy people make assessments of the need for specific research, but often they don't seem to be able to agree on relative priorities in allocating resources to or within forestry research.

DAVID LEWIS

You talked about the benefits to managers of research; having worked with research evaluation for about 6 years with Weyerhauser, I'd have to say that we did a very good job of doing it as managers. One thing we did accomplish was to make the scientists aware of what their science was doing to the business they supported and by the same token to give the business a better understanding of the role of technology. I am not sure that we managed any better, but the players were better informed and maybe that ought to be a very significant role for research evaluation in terms of our profession.

RICHARD SKOK

There are many levels of research evaluation. The annual evaluation of a project at an ag experiment station, or the annual evaluation of scientists at the university represent forms of ongoing research evaluation.

ROBERT BUCKMAN

How do users foresee the forestry needs? There was a series of four regional conferences and a national conference, and there were a number of policy issues addressed in those conferences. But there was also a workshop in 1976 at Airlie House, just outside of Washington, DC, that specifically addressed policy questions. One of the things identified in that workshop had to do with competitive grants, and the workshop participants thought that forestry research ought to have a competitive grants program. A couple of years later, the organic legislation of Forest Service research was rewritten. The prohibitions were inserted into it for competitive grants. At the same time the old science structure in the Department of Agriculture was modified by Title 14 of the 1977 Farm Bill, which also wrote competitive grants, and that was reaffirmed in 1981. We would have nominated competitive grants for funding a lot earlier if competitive grants over in the Agriculture Committee in Congress weren't in such trouble. But the Committee chairman was giving competitive grants a very bad time, and the deans of agriculture were not very happy with it, so we did not do anything with it.

This past year I was visiting with a Congressional staffer and he told me that he was concerned about something still floating around in the budget process called centers of excellence. I told him that one of the ways to meet the requirements for centers of excellence might be through the competitive grants route. The next thing we knew the Senate reported out $10,000,000 in the research budget for competitive grants. That whole sequence of events, that whole evaluation, was a judgmental kind of evaluation that went back to that Airlie House workshop in 1976. Sometimes these things have a fairly short incubation period.

Evaluation takes several forms. It takes the form of periodic review with an experiment station or university by the Cooperative State Research Service. It takes the form of some Delphi techniques. Ruttan commented on this judgmental kind of evaluation.

Dick Skok and I co-chaired, a number of years ago, at Grey Towers in Pennsylvania, a workshop on basic research in forestry. There were distinguished researchers from industry, universities, and the Forest Service, and we produced a paper that dealt with basic research needs in forestry. It took that paper about 2 years to reach fruition. But the committee staffs and others asked to see copies of that paper and during our budget year Congressman Yates started to go through that basic research paper. He was going through it paragraph by paragraph, and he said "Dr. Buckman, is this true?" And then he read another paragraph, and asked again, "Is this true?" Obviously there were politics floating all through that sort of thing. The next thing that happened was the Appropriation Committee added $700,000 to our budget for basic research and biotechnology. So evaluation takes a variety of forms and I concur completely with Fedkiw that we've got to keep evaluation in front of us, and that the time for more quantitative evaluation is here. But this is not the only kind of evaluation that counts. Some of the things that don't lend themselves very well to analysis are important too. Acid rain, a dominant feature in our budget right now, is a good example.

Our safety and security in the resource area depends on examining these questions. The risks of hazards will cause this, and we have to do something about it. If we don't protect ourselves against these kinds of risks, they may be upon us and we are less prepared. Congressman George Brown, who is "Mister Science" in the Congress, and was at the last joint council meeting, said that the Congress will give you hell if you don't solve today's problems. He said the Congress will give you hell if you don't deal with those problems, but they will also give you hell if you don't anticipate an unexpected problem down the road. So Brown went on to say, since you can't win you might as well do what is right. About that point he stopped giving advice. He left very much open the question about what is right.

GARY EVANS

I'd like to reflect a little bit on Ruttan's comments about the evaluation of "tremendous agricultural successes," such as hybrid corn. He tossed the question to forestry, asking "What do you have that you can capitalize on along the same vein?" We don't have that kind of 15-year story of effects of basic and applied research that could be put together to show the multi-percentage increase.

There is a supportive comment that goes along with forestry, and this goes back to what Fedkiw refers to as ex ante *evaluation. There are many research areas in forestry right now, from forest products to forest biology, where it would perhaps pay to use some of the* ex ante *evaluation techniques to look at what future effects can be and let history be the judge. It's from some of these areas that, as we go about designing not individual research projects but research areas, the evaluation techniques that Mike Schriven wrote about almost 15 years ago can be put to play. This requires designing the research program so that what he calls pre-evaluation can be conducted as the research project plan moves forward. You look at it from the standpoint of 25 years from now—how can we evaluate what we are starting to work on now?*

*Bob Callaham basically took what material he could find and did a retrospective assessment, a very unsophisticated one, but he proved his point. Forestry research has done some spectacular things. Between these two areas (*ex ante *and* ex post *evaluations) we sit in a unique situation, especially with this group here, being able to look ahead into some of these areas and say, as we design this program for forest products research or fire research or for soil biology, how can we design it so that we can look at the end result? We have the opportunity to build this into an evaluation design.*

BILL HYDE

I am often troubled by suggestions of that sort, not because there is anything at all the matter with that suggestion but because such suggestions can quickly deteriorate. They are likely to deteriorate to a state where we say we cannot do anything because we don't have data. It strikes me that as social scientists, most of us are used to searching for and using secondary data. There are some people here who conduct surveys, and perhaps that's primary data. The rest of us are used to using secondary data, and my observations are that there are usually ways we can get at the results we are looking for with second-best data. I think we have sensible results. It is not as easy as what could have been if we could have designed the data 30 years ago, and to that extent your point is absolutely correct. But we really have to be careful overstating data problems.

CHRIS RISBRUDT

Dick, you said something about evaluations being useful to decisionmakers, and I only caught one criterion, and that was credibility. Are there others that you look for in a useful evaluation that you could categorize?

RICHARD SKOK

If it doesn't have credibility, none of the others matter.

CHRIS RISBRUDT

What if it does have credibility, then what criteria are important?

RICHARD SKOK

It depends on the decisions being made and what kind of information you're working with. Basically at the program management level I don't find a great deal of information from research evaluation that's useful. At the level of formulating a budget request for Federal funding, there are useful gains in research evaluation. Criteria such as relevance, timeliness, and applicability are involved.

DAVID LEWIS

You mean that in the sense of justification, but you do not have much to tell us about how much we ought to be spending.

RICHARD SKOK

You are really working on political problems in working out the question of the level of program support.

BILL HYDE

You don't want a researcher to tell you that your level ought to be X hundreds of dollars. But a researcher could suggest that he has evaluated two or three different problematic areas and that ex ante *evaluation suggests that area one looks considerably better than area two. Then the senior administrator would have to decide how much more emphasis to put on area one. Choosing the support level is not something that you want an economist with a computer to do for you.*

RICHARD SKOK

It is very important to look at the level of decision-making that you're influencing. At the program level, the really critical decision at any point is program direction, particularly when the opportunity to hire a new faculty member occurs. Once you have the faculty member on board, there's relatively little you can do to influence his or her research program direction, because people tend to do research in the areas that they know something about. So the hire becomes a most critical decision. Now if you move up a level, and are looking at funding a research program with Federal monies, you've got options among programs as to where you put your dollars unless you are talking about formula-funded programs. Research evaluation can tell you about where the priorities for competitive awards ought to be. Then the system will allocate the dollars, theoretically, at the place and with the people most capable of doing the research as judged by peers using scientific criteria.

DENVER BURNS

It often strikes me that the difference between what the Administration proposes as a budget and what Congress appropriates deals with areas where we don't have quantitative information. We talked about utilization, and it is an attractive area because there are a fair number of things that you can get your hands on. But a major area, at least in the Northeast, that the Congress has been interested in in a different way than the Administration, is the environmental protection and environmental information that allows current management practices to go on. Among the areas where there are substitutes, something perceived to be environmentally more acceptable will replace the predecessor. An example, is the use of BT, a biological control for gypsy moth, in lieu of chemical insecticides. The chemicals do a great job, but the people in the Northeast don't want to see them used. There are no savings in using BT, in terms of dollars and cents, but in terms of public demand and desire, there is an extraordinary difference. I don't know how we capture that as a significant component in deciding the direction that research takes in terms of absolute dollars. Tradeoffs in values are totally separate from dollars.

JOHN FEDKIW

It's an income distribution question. Some people value some things more than others, and when new research becomes available, that changes the information base, and people's perceptions change.

ALLEN LUNDGREN

This is an interesting area. We sometimes forget, especially the economic analysts, that there is more than one value system out there. Different groups of people have different value systems and judge things differently. To some, economic analysis of research has no meaning whatsoever, and they would judge research, as you say, on their perception of whether this is good or bad for the environment. I think that in any research evaluation, we have to recognize the fact that we are dealing with different bodies of constituents who may react quite differently. You have to take a broader look at research evaluation and try to address some of these issues that may end up being critical in terms of acceptability of a program. You

can take a much broader look than just looking at it from an economic standpoint.

JOHN FEDKIW

When we use market data for evaluation, we are using the opinion of many, many people. Those market prices are established by transactions agreed upon by those suppliers and users. Those market values represent real values, widely agreed upon, otherwise they would not be market values. We don't seem to have that understanding, and somehow think that an economic analysis comes out of a set of principles. No, we are measuring the value as agreed upon by many people in many transactions. Where we have market values they ought not to be put down. As a matter of fact, they maybe ought to have more weight than the opinion of half a dozen people. And that is one of the things that is not understood because economists have been trained first in principles, rather than factual observations with which biologists and physicists work.

ROBERT BUCKMAN

I will argue that the invisible hand is operating in the allocation of research as well.

JOHN FEDKIW

I don't say that the other values and biases don't have a place, because that is what the market is about. It takes facts, lies, distortion, opinion, wishes, and wants, mixes them all together, and somebody votes. And they don't tell you why. The market place also accepts facts. It's just a question of how they are weighed by the people who vote. And that is what the market is about.

DENVER BURNS

This market economy you talk about draws on available products and available supplies and demand. It's not clear to me how, within the research sector where new information is being created, that can be factored into a free market.

JOHN FEDKIW

It works through the Patent Office and patent law protects you against the free market loss. We lose some research because we don't want it to be protected that way. We want it available to all the people. We have an ambiguous kind of society in a way; we do some things one way and we do some things the opposite way. We encourage private enterprise in research by granting patents. We have even encouraged the private sector with legislation that protects them in the adoption of technology that is publicly available to everybody so the adopters will not be out-produced by other people once they get it into the market. Adoptors of unrestricted patents can lose the advantage of earning profits without some protection.

DENVER BURNS

The point I was trying to bring out, using BT as an example, was that somebody took a real risk way back, and decided to research this area even though we had DDT and chlordane and other insecticides that effectively control the gypsy moth. Somebody still thought we really ought to put some dollars into BT, despite the fact that we already had some good controls.

My interest in bringing up this subject is to point out that the evaluation of BT would show that it doesn't save any dollars at this point, at least, in terms of some of the alternatives that might be used. Yet there is something dramatic going on. The public is saying, despite the fact that there is little cost-benefit, we prefer BT. In fact, this past year there has been a cost increase in BT.

JOHN FEDKIW

So that is part of an evaluation. You give the factual data and say, well we don't gain anything with this, any net, visible, social, measurable gains. It is a perceived value although it may not be real.

CHRIS RISBRUDT

Denver's point is that for research evaluation you are in a very difficult area for values. And a significant portion of forestry research may lie in that kind of area, difficult to value.

HANS GREGERSEN

They are not in an impossible area, except from the standpoint of the traditional view of the economist looking at economic efficiency. It is just another one of the nonmeasured benefits you have to sort out, such as a perception of avoiding health problems.

Outdoor recreation is another area. Foresters have spent a lot of time just sorting out some of the numbers, whether what you come out with is some kind of cost-benefit analysis or not. I think that kind of evaluation is useful.

RICHARD SKOK

How does that evaluation problem differ from the one that we are encountering as we push more and more into basic research?

HANS GREGERSEN

The more basic the research, the more you are talking about peer evaluation of the capability of the individuals in terms of the fundamental science they are dealing with. Here we are talking about something that still has an outcome in terms of people wanting this for health or other reasons. In basic research, from my point of view, we just have to rely on a peer group evaluation of the scientists.

RICHARD SKOK

How about the issue of the level of basic research?

HANS GREGERSEN

Dick, you made the point about credibility. People who are involved in the basic sciences stay outside the broader system, in a sense; these people have been going down the road looking for their pats on the back from their fellow scientists. Credibility is established in the eyes of their peers. In the applied sciences, a researcher's credibility has to be established in the eyes of the administrator and potential user of the adopted research. Similarly, credibility with regard to research evaluations has to be established in the mind of the user of the evaluation.

GARY EVANS

What is your definition of credibility?

HANS GREGERSEN

Very simple, something that is accepted by whomever is making the decision. If you think I am credible, you'll use what I do. The definition depends on the user's perception.

I think the first thing that Dick or any decisionmaker would do is try his darndest to find something wrong with that study. But if he is using it to make it a justification statement, and it comes out positive, that still doesn't mean he is going to say it's credible. He is going to try to weigh what the people who are against him will be saying about the person who did the evaluation. I remember one of the things we ran across in reviewing research evaluation methods was a statement from a Congressional oversight hearing. Question: What was most important to Congressmen as they looked at these thousands of studies that came across their desks? Answer: The number one thing was who did the study. That is the first thing they looked for. If it's a negative result, the decisionmaker is really going to look at it, because then the program must really be bad. Other than that, if the evaluation is from an interest group and it comes out positive, they just file it away. It's similar to when you hire somebody, you get reference letters. If you get a bad one you really notice, but if it is a good one, obviously the candidate has asked people who support him. The same credibility is extremely important, but I am not sure that scientific validity in the analysis necessarily is always tied up with credibility issues.

GARY EVANS

The other thing that needs serious consideration, and is extremely difficult for the scientists to agree to, is that we need more evaluations of projects that failed. That failed miserably. This gets back to part of the decisionmakers' information base. Why did they fail? What are the problems that caused this thing to blow up? We have lots of successes, but in the evaluation area, nobody wants to drag the dirty linen out. There are some valuable lessons to be learned and some information from the standpoint of program managers and administrators that needs to come out.

JOHN FEDKIW

This is one of the characteristics of ex post *evaluation of aggregates and subaggregates. It buries the failures in the wealth of success.*

ALLEN LUNDGREN

In a marginal analysis, the anatomy of a failure could teach you a lot about how you might be able to improve the chances of success, and perhaps avoid certain kinds of failures. There is no question in my mind that we should concentrate on our successes, because if you can't justify research on your successes it is highly unlikely you could justify it on your failures.

JOHN FEDKIW

ARS did some ex post *evaluations on individual projects and when they got all through with the numbers and so forth, they said, so what? There was one interesting one they did on avian leukosis, which is one of the poultry research projects. They spent a lot of money for about 20 years. All of a sudden it started to break and they got results. The research results had enormous impacts on the poultry industry in the form of a vaccine.*

But the breakthrough was that a scientist discovered they were dealing with two diseases instead of one. And when they sorted it out, they could do the research on each and get consistent results. Before, research findings apparently just didn't come out consistently. And that seemed to be the key. I don't know how useful the information is that you might get on a project-by-project focus. We have to do a little bit of that to learn how to do program evaluation, but not a great deal of it. Ex post *evaluations of many small projects is not particularly useful for decisionmaking.*

LYNN MAISH

That was a warning that if you would have done a study somewhere before the breakthrough, you might have concluded that they wasted 20 years of effort.

DENVER BURNS

I think this is a point where the interests of people who manage differ from those of the people who are involved academically. That is, I think that all of us in our experience have seen and maybe participated in failures, to acquire a sense of what those are about.

ALLEN LUNDGREN

I have seen some generalized figures on an industry level of the analysis of research and its final implementation. These generalized figures said about 10 percent of all research projects actually produce something worthwhile, and 10 percent of those may actually pass any kind of pilot-testing stage, and of the final ones that get implemented perhaps only 10 percent of those actually are booming successes in the marketplace. But it seems that we sometimes lose sight of the fact that almost all research is a gamble. There is no way to really know in advance whether or not a particular research project or program will be successful. If you look at enough of them you can talk about probabilities of success. But you can't attach a probability of success to any one of those things. Only a certain proportion of them are going to be successful.

ANNE FEGE

But it depends on how conservative or liberal you were with the definition of the research problem, and how high a risk you are willing to take when you set it up. If you are consistently conservative you are going to have few failures.

DENVER BURNS

We strengthen and support that by studiously avoiding duplication, which is a major component of the research of companies mentioned in "The Search for Excellence." It is taboo to have duplication in agricultural research. That comes from legislative agreement.

JOHN FEDKIW

Some of that should be decided by the managers, not by the scientists or evaluators. What evaluation does is produce information useful for decisionmaking. Usually the scientists are not going to make a decision. They may not have a full concerning or a good perception of what information is going to be most useful for specific decisions at some particular time or date. In the budget cycle there comes a time when information is useful and when you pass that day, you wait another year before you can make use of it. That is the kind of thing we haven't talked about here. I believe it will be important when we get to tomorrow's discussions, which concern future directions. Scientists are not really the best judges about what we should evaluate and for what purpose. It is really something that should be determined by policy people, the people who make resource allocation decisions.

HANS GREGERSEN

That depends on the level of decisionmaking. At the project level there is information needed, too. I would predict quite strongly that at a project level, researchers are the ones who are making the decision. That may be an area we have ignored—that is, evaluations useful to researchers making their decisions, as they go from research project to research project, as opposed to a program policy or level.

JOHN FEDKIW

Ex ante *evaluation is selected to be project-oriented, or to address a group of projects that have the same end. But in the first 5 years of this research evaluation project, the decision was to focus on developing methodology, and the involvement of scientists in choosing what they were going to do was very high. Probably this is the right thing to do. But as you move forward with this evaluation effort, there is enough capability and capacity involved so that we ought to be thinking, where is it going to be most useful? The information aspect, rather than the methodology aspect of evaluation, is going to be more important the next cycle, and we should weigh that a little bit more.*

HANS GREGERSEN

I think that is a very good point. I think we have all the techniques we need. What we really have a problem with is matching the technique with the kind of information that is available.

RECENT AND CURRENT RESEARCH EVALUATIONS

A REVIEW OF CURRENT FORESTRY STUDIES

Chris Risbrudt,
USDA Forest Service,
North Central Forest Experiment Station,
St. Paul, Minnesota

In this session, we want to review and comment on the studies in research evaluation that are recently completed or in progress. Also, we want to talk about how these studies that are now in progress fit into the needs and uses that the previous speakers discussed. Joining me in this review this morning are Hans Gregersen and Bill Hyde. We picked these two because they have the most graduate students in this area, so we know there is some work being done, anyway. I think we have good graduate students working on these problems, and generally they are doing some excellent work. I would like to talk about the three problem areas in Research Work Unit 4204 at the North Central Forest Experiment Station, and to briefly note some of the studies currently underway that are not under the aegis of these two gentlemen.

We have three problem areas in NC-4204, Methods for Evaluating Forestry Research, that we have recently been approved to work on for the next 5 years. The first is measures of research productivity or, more precisely, researcher productivity. Here we are reviewing one of the traditional ways of measuring scientist productivity: publication counts. We have been tabulating them for Forest Service researchers by publication-refereed journals, station papers, other outlets, non-refereed journals, books, and others. Each Station puts out an annual attainment report where, project by project, scientist years, funding by source, and publications that result are listed. At North Central for the past half dozen years we have been analyzing our attainment report to determine number of publications per scientist year and $100,000 of research investment for each project. We are hoping we can come up with more and better measures of researcher productivity, such as an input-output analysis of research. We are also evaluating other measures, such as citation counts; the latter is probably more useful in basic research than in applied, although we are looking at both.

Our project, working with the Northeastern Station, is looking at coauthorship patterns—a sort of who influences whom issue. This helps us identify the centers of productivity, excellence, or knowledge in various fields such as short-rotation forestry. We are also developing a data base of all Forest Service publications. We have attainment reports computerized for fiscal years 1980 and 1981; this listing includes over 4,000 publications. We have found, for example, that of the authors who produced more than four publications over those 2 years, 17 percent produced 47 percent of the publications. Hence, we are looking at high and low producers on that publication scale. Are there characteristics that differ among them? Did the high producers have more cooperative funding, did they have more technicians, were they part of a bigger or smaller unit? We thought it would be interesting to look at such factors. We are well aware of the limitations of indicators such as publication counts as a measure of scientist productivity. We are reminded of the two centurions who were in charge of the crucifixion. One said to the other, "I hear he was a very great teacher"; the other said, "Yes, but he didn't publish anything." So it is one way of measuring scientist productivity, but hopefully we can begin to look at others, too.

The second problem area that we are dealing with is the diffusion of research-generated knowledge, and what factors affect it. There has been a great deal of work done in this area in other fields, and the framework for this type of study is generally established, but we want to concentrate on some of the forestry innovations. It is tempting to look just at forest products utilization research to say, for example, how fast the power backup roll has moved into industry, but we also want to look at innovations in forest management, or forest insects and disease research. How fast does this new knowledge move out into the field where it is used, and what are the important factors affecting it? Research on the diffusion of research-generated knowledge will identify the link between researcher and user.

Our third problem area, and this is where we plan to spend about 80 percent of our effort, is in developing measures for research evaluation. Here I will briefly summarize some of the studies being done

around the country. I think that is the best way to explain the kinds of things we will be working on in this area. All of these are summarized in the appendix. Bruce Bare at the University of Washington is evaluating the impacts of the regional forest nutrition research projects in Oregon and Washington, and looking at its inputs and outputs and its impacts. David Lewis is just beginning a project at Oklahoma State to identify the regional impacts of new technology, as implemented by a major firm. There is a special opportunity to do this in southeastern Oklahoma, where Weyerhaeuser bought out Dirks in 1969. We have a situation where we can isolate and determine the role research played in that decision to move in and change from an extensive to an intensive management system. The company also rebuilt the mills. Jeff Stier at the University of Wisconsin is beginning a study on evaluating the impacts of genetics research in the Lake States, which is essentially evaluating the NC-99 tree improvement cooperative in the North Central Region. That cooperative has been operating since the mid-1960's. There are records of the costs and we can estimate some of the impacts of that research.

Joe Chang at the University of Kentucky is working on a value of information study. He is estimating the value of local growth and yield tables, and trying to determine the value to forest management of additional information. As a secondary study he has examined the new growth and yield model for New England red oak. Until this yield model was developed there hadn't been a local growth and yield table or model for the New England States; they were using Midwest information. Joe estimated what the better model is worth in New England and it is rather interesting. Steve Bullard of Mississippi State is starting an aggregate study of logging research, in which he hopes to compare the effects of research in the Pacific Northwest versus the South.

These and other studies now in the planning stages will begin to form a basis for forestry research evaluation, for developing the necessary methods, and for identifying and quantifying the impacts of new knowledge. Improved knowledge of the research process itself, and its role in meeting the goals of society, should result.

THE UNIVERSITY OF MINNESOTA FORESTRY RESEARCH EVALUATION PROGRAM

Hans Gregersen,
College of Forestry,
University of Minnesota,
St. Paul, Minnesota

I will very briefly describe the studies that the College of Forestry, University of Minnesota has carried out over the past 4 years, supported with funds from the Minnesota Agricultural Experiment Station, the North Central Forest Experiment Station, the Forest Products Lab, and the USDA Cooperative State Research Service. Ten people have been involved in this effort. Table 1 summarizes the program, the specific topics addressed, and the persons involved.

Allen Lundgren was instrumental in the initial discussions and the initial choice of how to approach the problem of forestry research evaluation. We looked at it very broadly and decided the obvious thing to do first was to see what had been done in the

Table 1.--University of Minnesota forestry research evaluation program[1/]

A. What has been done in the past and how has it been done?
   1. Survey of methods used to evaluate research: Development of a framework for analysis.
   2. Bibliography on research management and evaluation.

B. What is involved in actually applying some of the methods?
   3. Returns to research on structural particleboard.
   4. Historical review of structural particleboard research.
   5. Returns to research on containerized seedlings.
   6. Intermediate inputs and technological progress.
   7. Factors associated with productivity growth in the U.S. forest products sector: A survey of opinions.

C. What is the nature and process of diffusion of forest research results? How does technology transfer fit into the picture?
   9. Diffusion of forestry research results.
   10. Technology transfer in the U.S. Forest Service: Some observations.
   11. Income redistribution and the value of forestry research: Issues and approaches.
   12. International technology transfer and forestry research.

D. Who needs evaluations? What do they need them for? What do they need?
   13. Forestry research planning and budgeting and evaluation needs.
   14. International comparative study of research management and planning approaches of IUFRO members.
   15. Comparative study of research management and planning approaches of U.S. IUFRO members.
   16. Approaches to estimating optimum forestry research budgets.

1/ Persons from the University of Minnesota directly involved in one or more of these studies include: D. Bengston, D. Current, H. Gregersen, J. Haygreen, I. Holland, A. Hyun, M. McDill, J. Olmstead, A. Strees, R. Westgate.

past in other fields and how it had been done, not just in forestry, but in research evaluation in general.

So the first study we started on was a survey of evaluation methods used in different fields (Gregersen *et al*. 1983a). Using this review, we then developed the framework for our future studies. We started to accumulate and digest a great deal of documentation, so the second logical project was to develop a bibliography on research management and evaluation. We made it rather broad because a lot of what we were finding dealt generally with how managers go about generating and using information to make decisions. The bibliography is now available (Westgate 1984a). It represents a drawing together of all the materials we had found in this area of research evaluation, broadly defined.

As we proceeded, we started getting uneasy about just looking at approaches without knowing what the problems are in applying them. So we decided to do some actual research evaluations to see what they really are—what's involved in them and what kinds of problems one runs into. Since most of us are economists we decided to start with economic analyses, particularly applying some of the ideas developed in the agricultural research evaluation field.

The first of our empirical studies looked at the returns to research in structural particleboard. Bengston undertook this one; he came out with rates of return between 19 and 22 percent (Bengston 1984). The main problem he ran into was lack of data, and the key challenge was to adapt approaches to availability of data. At the same time, together with John Haygreen and Andrew Hyun, we started on an historical review to trace the process of research in relation to the development of structural particleboard (Haygreen *et al*. 1985). We basically looked at the documentation on this research and linked it to what has gone on in actual practice in the structural particleboard industry. The next study was an evaluation of research on containerized seedlings. We have the preliminary results, again showing the kinds of rates of return you find in agriculture. Again, one of the main problems in this study was getting some basic numbers to put into the evaluation (Westgate 1984b).

Another major analysis was included in a study of forest products research that John Haygreen, Irv Holland, and I completed with assistance from Dan Erkkila. Our idea was to take all costs associated with forest products research—broadly defined to also include forest products marketing, logging, and engineering—and to compare these costs with the benefits of seven major innovations, admittedly seven very good ones.

We tried to see if the benefits of the seven could justify the entire research cost for the whole program area. Thus, we dealt with the common criticism that research evaluators only pick "good" projects, since we included the costs of all forest products research in our comparison. Depending upon what specifics were included, the results were internal rates of return in the 18 to 30 percent range (Gregersen *et al*. 1983b).

Other papers coming out of the University of Minnesota forestry research evaluation program deal with forestry research and income redistribution, intermediate inputs and technical change in the U.S. lumber and wood products industry, and aggregate returns to lumber and wood products research. Various manuscripts have been prepared and submitted for publication.

I think that we have already learned a lot from the evaluations in terms of the problems that exist. It is clear that we can't just sit back and look at methods in isolation. We also have to concern ourselves with building up a utilizable data base in forestry.

Another question we are addressing at the University of Minnesota is the nature of the process of diffusion of forestry research results. How does technology transfer fit into the total picture of innovation, or getting new knowledge into use and applying science and technology in the forestry field? This topic is critical because of the way we define research benefits (like John Fedkiw did much earlier): You don't have benefits until research is in use, in a pragmatic sense. Therefore, it is obviously important how fast those results get into use and how widely they get used. The first study under this heading is a general review of the literature dealing with the diffusion process and how it relates to forestry (see item in this proceedings).

Another topic being addressed is international technology transfer and forestry research. This is a study we are just starting. We have done a review of the literature, but the basic idea is to see how research from other countries is used in this country and how U.S. research results are used overseas. What kinds of things can we say about these relationships and what are the implications in terms of how the Forest Service or universities should be developing formal means for increasing certain types of international technology transfer or access to technology?

Another project we are working on deals with the who, what, and why of evaluation. We felt that before we really can make much sense out of what analysts can contribute to managers and decision-makers, we need to review past use of evaluations.

What do managers want them for and what information do they actually need from an evaluation? To a great extent, that is what this meeting is about. Obviously, in forestry the answers relate very much to decisionmaking, to justifying decisions, and to making better allocations.

A comparison of research management and planning approaches of International Union of Forestry Research Organization (IUFRO) members is being completed. It involved sending out questionnaires to IUFRO member countries and institutions, and asking for information on research planning, evaluation and management, broadly defined, with some specific questions related to use of evaluations and evaluation needs. We have had an extremely good response rate (78 percent), considering that some of these countries don't have much capacity to spend time answering questionnaires. We received 117 responses from 58 countries. A paper on the results is available (Gregersen 1984). A very interesting result is the consistency across countries with regard to how they view certain aspects of research management and planning. One explanation is that a great number of these institutions are led by persons who were educated in the United States and Europe. In other words, people will come here or go to Europe to study and take back the management process and style and the research ideas of the countries in which they study.

A similar study of the U.S. IUFRO members is also being completed. Again, an extremely high return rate was achieved (89 percent). All universities except one responded. One of the things we want to do is to go back to the responding institutions and try to develop more detailed and concrete ideas of why certain results occur and what they mean in terms of research organization, evaluation and planning.

Finally, the last study I will mention is a look at the approaches to estimating optimum forestry research budgets. Again, we are drawing heavily on what has been done in agriculture. Literature and approaches are being looked at and a model is being developed. A thorough review has been completed by Glenn Fox of the Department of Agricultural and Applied Economics here (Fox 1985).

Some of the other papers produced in our program are included in this volume (cf. papers by Bengston, Olmstead, Westgate). We feel we have learned a lot from these diverse, yet interlinked, efforts.

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A PROPOSED FORESTRY RESEARCH EVALUATION PROJECT

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Allocating research dollars is a new topic of inquiry for forest and resource economists. Such decisions are usually made by politicians, managers, and administrators. In recent years, however, tighter budgets for both public agencies and private corporations, greater scrutiny of the public planning process, and better analytical tools have led to a more organized and disciplined inquiry into research budgeting. This raises natural questions for economics, itself a discipline of resource allocation. They are difficult questions, however, because research is a sector where the pricing system works imperfectly, if at all, yet the pricing system is the basic system on which economic analysis relies. Fortunately economists from related fields, notably agriculture and industrial organization, have struggled with similar research budgeting and allocation problems and can assist us with their insights.

The profit motive has always justified internal scrutiny of *private* research budgets, and such scrutiny is more important during financially difficult times—as the years since 1978 have been for the forest products industries. The higher profile of *public* research budgeting can be traced from a variety of recent events. Critical issues in public agricultural research were the topic of a high-level workshop jointly sponsored in 1982 by the Rockefeller Foundation and the Federal Office of Science and Technology. The Renewable Resources Planning Act of 1974, as amended in 1976, and the Reagan Administration have stimulated inquiry into public forestry research expenditures in particular. The former requires a decennial assessment of forestry in the United States, together with a planning document that includes planning for forestry research. The latter, through both the Assistant Secretary of Agriculture for Natural Resources and the Environment and the Office of Management and Budget, has expressed doubt regarding the benefits originating from public forestry research expenditures—thereby encouraging more careful measurement of these benefits. In partial response, the U.S. Forest Service established a major new research work unit to explicitly inquire into this issue (Research Work Unit FS-NC-4204, Methods for Evaluating Forestry Research). Simultaneously, two sections of the International Union of Forest Research Organizations identified research evaluation as their fundamental agenda item (S6.06-01, Evaluation of Forestry Research, and S4.05-01, Evaluation of the Contribution of Forestry to Economic Development). Finally, the Society of American Foresters acknowledged the timeliness of the topic by making "Increasing Forest Productivity" the theme of its 1981 national convention. Recognizing that research is the key to increasing productivity over time, that convention included important sessions on research evaluation and research productivity.

This paper is a consequence of this background of interest. It introduces a research program currently underway at Duke University. The overall objective of this research program is to assess the economic returns and the distributive impacts of research investments in forestry. Economic returns may be monetary as well as nonmonetary. Distributive impacts include both allocation of research benefits and costs among factors of production and allocation between producers and consumers. We intend to begin our research with a review of the institutions and mechanisms active in forestry research. We approach our objective initially with an investigation into the payoffs resulting from the aggregate of all previous forestry research and then follow up with a sharper focus on certain specific forestry research projects. The specific examples, in research areas like softwood plywood production and genetically improved seedlings, should serve as illustrative cases providing understanding of previous successes and failures as well as insight useful for future research budgeting and program planning.

Our analytical methods call on the experience of agricultural economics modified to fit the requirements of available forestry data. These data are often less precise as to geographical origin and as to specific factors of production than are comparable

agricultural data. They may also have been collected for shorter time periods. This latter difference is of particular importance when one considers the large difference in length of the production periods between forestry and agricultural products, therefore the general requirement for longer sequences of forestry data in order to draw conclusions equally as valid as those from agricultural data. Our intention is to make the methodological modifications required by these data differences of a general nature such that they may serve as guides for our empirical analyses as well as for subsequent evaluators of other forestry research projects.

Forestry research expenditures originate from both the public and private sectors, but there are several reasons to expect the public sector to dominate. A large degree of risk or uncertainty discourages private investment regardless of the expected payoffs. The long time period between biological research investments and new knowledge realized in a growing stock of timber—and finally, an expanded harvest—suggests a substantial degree of uncertainty since both the production processes and the future markets are difficult to predict. The generally small size and small market shares of most firms in the forest products industries may be the key to other factors discouraging private forestry research investments. Indivisibilities in research effort suggest a large minimum scale for research operations which in turn implies high research entry costs relative to the small size of forest product firms. Furthermore, small market shares suggest that innovative firms may be unable to claim a large proportion of the gain from their innovations. Rather, the gains from innovation may spill over to so many other firms that the spillover gains to other firms may eventually be greater than the initial gain to the innovator. We might expect small market shares to be a substantial deterrent to private research investment in the timber growing, lumber, and furniture industries, although it may be less a deterrent in the more concentrated and capital-intensive pulp and paper industry. We will test these hypotheses about firm size and private forestry research activity in the future. Meanwhile, these hypothesized factors, taken together, cause us to show greater, although not exclusive, attention to publicly funded research. This attention is also consistent with the authors' own more general concern with public policy than with the internal operations of private firms.

With this foundation of intent; that is, an objective of determining returns to investments in forestry research using appropriately modified methods from experience in agricultural economics and with some special attention to the public sector; let us consider the additional background information which this introduction can provide.

The remainder of this paper divides into three parts: background, expectations, and an introduction into the structure of the research project itself. The background section introduces the general findings of others regarding the benefits of research in agriculture and other industries. The substantial magnitude of these findings suggests, by implication, further justification for our effort. A brief survey of previous analytical methods identifies the problems encountered in the literature, and thereby anticipates problems we may encounter and recommends their solutions. This background also introduces two topics related to the benefits of research investments: technical change and technology assessment. The expectations section reviews economic theories of growth and development as they pertain to American forestry. These theories and their empirical tests provide hypotheses for our study of forestry research. The hypotheses can be seen as hints as to where we might observe the most productive forestry research experience in the past or the most productive research opportunities in the future. They also cause us to inquire into the characteristics of successful forestry research. The paper closes with a brief "guide" to the anticipated analytical components of our full research project.

BACKGROUND

Technical change is the generalized product of research. That is, research is the input and technical change is an intermediate product which, together with other inputs (land, labor, and capital), fuels the economy. Its impact is measured as the change in economic output which would occur if both the quantity and quality of all other inputs were held constant. We begin this section with a few words about the technical change literature before turning, first, to the empirical findings of the returns to research literature and, then, to the accepted methods for calculating returns to research. This methodological review will be ever-so-terse since its intents are only to relay general concepts to the reader and to acquaint him/her with the most basic analytical problems. Those with greater technical interest are referred to the methodological survey by Norton and Davis (1981).

Making the best use of resources at any moment in time is important but, in the long run, it is dynamic performance that counts. For example, consider an increase from 3 to 5 percent per annum in the rate of economic growth due to technical progress. It overcomes, in just 5 years, the effect of a static output constraint causing a 10 percent reduction in potential gross national product. An increase in the rate of growth due to technical progress from 3 to 3.5 percent overcomes a 10 percent static output effect in

20 years. Thus, we can say that factors contributing to economic *growth*, in time, explain a large share of the *static* level of aggregate output as well.

The underlying significance of technical progress or technical change is that it substitutes new knowledge for less abundant and more expensive resources: land, labor, and capital. Research is the input that produces this new knowledge, and thereby yields increased returns to land, labor, and capital by (1) decreasing their costs and increasing their outputs, (2) increasing the quality of production and introducing new production processes, and (3) decreasing producers' vulnerability to external forces like pests and the weather.

Solow's (1957) work responds to these ideas. From 1909 to 1949, the measured United States' average increase in nonfarm productivity per worker-hour was 1.5 percent per annum. Solow found that 19 percent of this 1.5 percent was due to expanding capital intensity (i.e., more machines per worker). The remaining 81 percent of annual growth in nonfarm productivity was due to (1) technical improvement in productive inputs and productive practices (technical change in its strictest sense), and (2) increased quality of the labor force.

Denison (1974) extended this work for the years 1929 to 1969. He found that 22 percent of nonfarm productivity growth per worker-hour was due to improved education of the labor force, 48 percent to advances in scientific and technical knowledge, and only 12 percent to expanding capital intensity. Solow's and Denison's conclusions are that growth in output per worker in the U.S. originated predominantly from the application of ever newer, superior production techniques by an increasingly well-trained work force. Furthermore, their conclusions are conservative to the extent that Solow and Denison could only measure process innovation and not product innovation. Product innovation refers to changes in the quality of aggregate national output. There is no satisfactory measure of output quality changes, yet they clearly occur and as a partial result of technical change and improved labor quality, both of which are at least partially the products of research. Therefore, Solow's and Denison's impressive measures of the impacts of technical change are underestimates of the impacts of research-related activities on economic growth.

Knutson and Tweeten (1979), in extending the original methodologies of Evenson (1968) and Cline (1975), made a related inquiry into the productivity of research expenditures. They, however, restricted their attention to agriculture, specifically to all U.S. agricultural production-oriented research and extension for the years 1939 to 1972. They found a 50 percent annual internal rate of return on the marginal research and extension dollar spent in the decade from 1939 to 1948. This rate of return has tapered off since 1948. Nevertheless, it remained an impressive 35 percent for the years 1969 to 1972, the final period in the Knutson-Tweeten analysis (Evenson *et al*. 1979).

These studies examined either economy-wide technical change or the impacts of aggregate research expenditures. There have also been numerous analyses of the impacts of more specific research problems, many of them agricultural research problems.[1] Griliches (1958) initiated this micro-analytic work with his investigation of the social returns to research expenditures on corn hybridization. He found an annual internal rate of return in the neighborhood of 35-40 percent for the period 1940 to 1955. Brumm and Hemphill (1976) and Ruttan (1980) surveyed the subsequent literature. A summary of Ruttan's more recent survey is reproduced as our table 1. His categorization of the literature as either "index number" or "regression analysis" refers to the analytical method used in each article. We will discuss these methods momentarily. Ruttan found 36 articles reporting returns that range from 11 percent for wheat research in Columbia to 110 percent for both cotton research in Brazil and rapeseed research in Canada. The vast majority of the articles which he surveyed reported returns exceeding 35 percent per annum regardless of the agricultural research problem or the country which served as focus for the research. The Brumm and Hemphill survey includes nine articles on nonagricultural research problems. Rates of return reported in these nine articles are consistent with those found in the Ruttan survey.

These rates compare quite favorably with private industrial internal rates of return, which typically average 10 percent or more on capital investments and fall to the range of 2 to 5 percent at the margin. Thus, in spite of possible measurement and estimation errors and various problems inherent in comparing rates of return which may sometimes be more average than marginal, the magnitude of difference between rates of return to research, particularly agricultural research, and rates of return to general capital investments is persuasive evidence of the underallocation of resources to research. It becomes all the more persuasive when we recall that these are returns for process innovation. They overlook the currently unquantifiable benefits of research leading to new or improved products. In sum, the next

[1]*There have also been a number of detailed case studies of particular innovations which trace out subsequent consequences. Mansfield and his colleagues (1968, 1971, 1977) provide notable examples. Griliches (1973) surveys these.*

Table 1.--Summary studies of agricultural research productivity

Study	Country	Commodity	Time Period	Annual Internal Rate of Return (%)
Index Number				
Griliches, 1958	USA	Hybrid corn	1940-1955	35-40
Griliches, 1958	USA	Hybrid sorghum	1940-1957	20
Peterson, 1967	USA	Poultry	1915-1960	21-25
Evenson, 1969	South Africa	Sugarcane	1945-1962	40
Barletta, 1970	Mexico	Wheat	1943-1963	90
Barletta, 1970	Mexico	Maize	1943-1963	35
Ayer, 1970	Brazil	Cotton	1924-1967	77
Schmitz and Seckler, 1970	USA	Tomato harvester, with no compensation to displaced workers.	1958-1969	37-46
		Tomato harvester, with compensation of displaced workers for 50 % of earnings loss		16-28
Ayer and Schuh, 1972	Brazil	Cotton	1924-1967	77-110
Hines, 1972	Peru	Maize	1954-1967	35-40[a]
				50-55[b]
Hayami and Akino, 1977	Japan	Rice	1915-1950	26-27
Hayami and Akino, 1977	Japan	Rice	1930-1961	73-75
Hertford, Ardila, Rocha, and Trujillo, 1977	Colombia	Rice	1957-1972	60-82
		Soybeans	1960-1971	79-96
		Wheat	1953-1973	11-12
		Cotton	1953-1972	none
Pee, 1977	Malaysia	Rubber	1932-1973	24
Peterson and Fitzharris, 1977	USA	Aggregate	1937-1942	50
			1947-1952	51
			1957-1962	49
			1957-1972	35
Wennergren and Whitaker, 1977	Bolivia	Sheep	1966-1975	44
		Wheat	1966-1975	-48
Pray, 1978	Punjab (British India)	Agricultural research and extension	1906-1956	34-44
	Punjab (Pakistan)	Agricultural research and extension	1948-1963	23-37
Scobie and Posada, 1978	Bolivia	Rice	1957-1964	79-96
Pray, 1980	Bangladesh	Wheat and rice	1961-1977	30-35
Regression Analysis				
Tang, 1963	Japan	Aggregate	1880-1938	35
Griliches, 1964	USA	Aggregate	1949-1959	35-40
Latimer, 1964	USA	Aggregate	1949-1959	not significant
Peterson, 1967	USA	Poultry	1915-1960	21
Evenson, 1968	USA	Aggregate	1949-1959	47
Evenson, 1969	South Africa	Sugarcane	1945-1958	40
Barletta, 1970	Mexico	Crops	1943-1963	45-93
Duncan, 1972	Australia	Pasture Improvement	1948-1969	58-68
Evenson and Jha, 1973	India	Aggregate	1953-1971	40
Cline, 1975 (revised by Knutson and Tweeten, 1979)	USA	Aggregate	1939-1948	41-50[c]
		Research and extension	1949-1958	39-47[c]
			1959-1968	32-39[c]
			1969-1972	28-35[c]
Bredahl and Peterson, 1976	USA	Cash grains	1969	36[d]
		Poultry	1969	37[d]
		Dairy	1969	43[d]
		Livestock	1969	47[d]
Kahlon, Bal, Saxena, and Jha, 1977	India	Aggregate	1960-1961	63
Evenson and Flores, 1978	Asia-national	Rice	1950-1965	32-39
			1966-1975	73-78
	Asia-international	Rice	1966-1975	74-102
Flores, Evenson, and Hayami, 1978	Tropics	Rice	1966-1975	46-71
	Philippines	Rice	1966-1975	75
Nagy and Furtan, 1978	Canada	Rapeseed	1960-1975	95-110
Davis, 1979	USA	Aggregate	1949-1959	66-100
			1964-1974	37
Evenson, 1979	USA	Aggregate	1868-1926	65
	USA	Technology oriented	1927-1950	95
	USA	Science oriented	1927-1950	110
	USA	Science oriented	1948-1971	45
	Southern USA	Technology oriented	1948-1971	130
	Northern USA	Technology oriented	1948-1971	93
	Western USA	Technology oriented	1948-1971	95
	USA	Farm management research and agricultural extension	1948-1971	110

Source: Vernon W. Ruttan, Agricultural Research Policy (Minneapolis: University of Minnesota Press. 1982.) p. 242-243.

dollar spent for agricultural research will yield a much greater return than the last dollar invested for capital improvements in private industry. Apparently there is opportunity for considerable social gain from additional investment in research. Many more dollars could be spent on research before its marginal return falls to a level comparable to that of marginal investments in other sectors of the economy.

The natural question for us has to do with whether *forestry* research generates similarly high rates of return, thereby suggesting a similar underallocation of resources to forestry research and social gain to expanding forestry research budgets. This is the basic question for this research. There have only been scarce attempts to respond to it previously (Porterfield 1974, Davis 1967, Pee 1977, Thomson 1983 and work in progress by Gregersen and his associates).

First, however, we would like to raise our own initial unsophisticated doubt. This doubt causes us to inquire into how these impressive rates of return for research were determined. Are the methods of empirical calculation valid? Our doubt is founded in our casual observations of both many researchers who never produce and many who produce but whose work is never widely implemented. We are sure that all readers of this paper have observed both groups and would agree that both act as drains on research budgets. This suggests that there may be overinvestment in research and throws doubt on methods yielding empirical results to the contrary. In response to our observations of poor performance, however, we must ask whether such underachievement is unusual. Perhaps underachievement occurs among researchers, but it also occurs in other sectors of the economy as well. Perhaps underachievement acts as a drag on investment returns everywhere (in research, but also in the production, distribution, and service sectors of the economy), and rates of return in research are as high as they are only because underproductive research effort is substantially offset by other much more productive effort from other researchers and other research projects, an offset which is perhaps not nearly so substantial in other sectors of the economy. In this latter case our casual observations of underachievement in research may suggest areas of local improvement in research budgets. These observations are not sufficient, however, to suggest gross misallocations on a more aggregate level. If sound analytical and accounting methods are used in the research evaluation literature, then our doubt must be laid to rest.

METHODOLOGY

Two general methods have developed for evaluating returns to research: the consumers' and producers' surplus/benefit-cost method and the production function method. We will examine these methods briefly here. We will look first at the consumers' and producers' surplus method with reference to the standard supply and demand functions of the economist as drawn in figure 1.

Figure 1 shows value per unit on the vertical axis and units of output on the horizontal. The quantity of product which consumers demand varies inversely with product price according to the function D. The quantity which producers are willing to supply increases with their unit production costs according to the function S. Economic equilibrium occurs, that is, the market for our product clears, where the supply and demand functions intersect at a, or where the price consumers are willing to offer for the last unit of product is p_1, which is also the cost of producing that unit. A total of q_1 units of product exchange in this market.

Total production cost for q_1 units is the area under the supply function up to the market clearing output level or $0aq_1$. This cost pays for the variable inputs in the production process; raw materials, labor, and capital. All units of output sell at the same market clearing price p_1, therefore, all but the last unit attract payments greater than their production costs. The total of these surplus payments, $0ap_1$, is the return to the fixed factor of production, often managerial skill, and is known as economic rent or producers' surplus. Although consumers only pay p_1 for each unit of production which they consume, units consumed prior to the final unit are worth more to them. Total value to consumers for q_1 units is the area under the demand function and to the left of the market equilibrating output level or $0q_1ad$. Total value to consumers, net market payments of

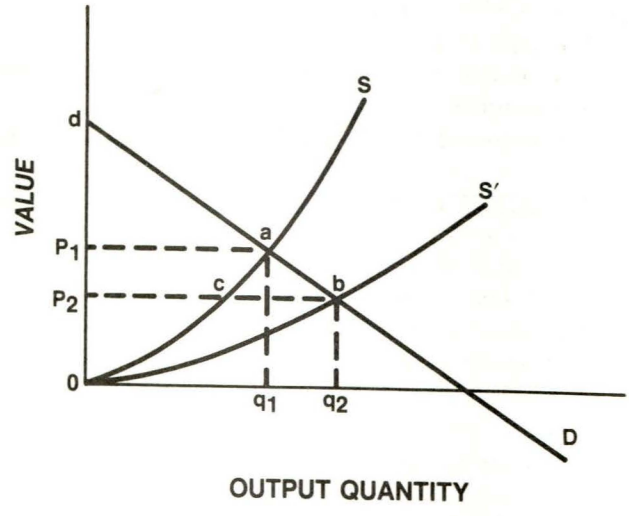

Figure 1.—*Consumer's and producer's surplus*

consumers' market, is p_1ad and is known as consumers' surplus. The gain to society from producing and consuming q_1 units of output is 0ad, or the sum or producers' plus consumers' surpluses.

The previous two paragraphs describe the static economic condition. When technical change occurs it alters much of this situation. Technical change ("process" oriented technical change to be precise) either alters the production process itself, thereby permitting use of a lower cost combination of the same inputs, or makes possible the use of a less expensive substitute input. In either case it decreases cost per unit of product such that the supply function shifts downward to, say, S' in figure 1. The new market equilibrium occurs at a lower price p_2, and a greater level of output q_2 clears the market. Technical change creates a gain in consumers' surplus equal to $p_1 abp_2$ and a loss in producers' surplus equal to $p_1 acp_2$, but a countering gain in producers' surplus equal to 0cb.[2] Technical change has no significant impact on the aggregate use of the inputs of this production process if the inputs all exchange in competitive markets and if the production process requires only a small share of the aggregate market availability of each input. In this case, factors which are released from production because of the technical change find employment in other markets and at their unchanged market prices. In conclusion, the net social gain due to technical change is $p_1abp_2 - p_1acp_2 + 0cb = 0ab$. This approach to measuring the gains associated with technical change is also known as the "index number" approach because much of the literature has concentrated on finding indices for measuring the relevant triangles in figure 1.

Benefit-cost calculations can be made for research projects using the consumers' and producers' surplus method. The annual benefits are the net social gains due to technical change (which are described above and in figure 1), while the costs are those associated with doing the research and implementing the technical change resulting from this research. One of the complexities of benefit-cost calculations is due to timing, or diffusion of the new technology over time and space. There is a large body of literature devoted to this issue, but in general we can say that research implementation costs occur in the initial year or years while benefits occur only after implementation and continue over time.[3] The common assumption is that once the new technology is fully in place, then the benefits begin—and continue unabated indefinitely. Future research and technical change only build on the basis created by past benefits. (This provides a conservative accounting of the gains to research because some technical change is necessary to maintain even a no-growth society. For example, immune treatments against pests and diseases deteriorate and must be replaced with new treatments, which are the results of new research.) Knowledge of the time streams of benefits and costs and the opportunity costs of capital permits calculation of a benefit-cost ratio for the particular research-induced technical change in question. Alternatively, in absence of a known opportunity cost of capital, many research evaluations search for the rate of return which sets the benefit stream equal to the cost stream. The result is an average rate of return for all dollars spent on the given research investment.

The second method for evaluating returns to research focuses on the production function. "Production function" is the economist's term for the physical input-output relationship described by function F in figure 2. As the physical quantity of inputs increases, then physical output must also increase, but eventually output per unit of input can increase only at a decreasing rate. Technical change makes possible an output increase using the same quantity of variable inputs as used before introduction of the new technology. It is described in figure 2 as a shift of the entire production function from F to F'. Observed output shifts would be points on these functions; for example, the initial output level q_1 using x_1 units of input may shift to output level q_2 using x_2 levels of input during a period in which technical change occurs (and quantity demand increases).

The shift in the production function must be carefully distinguished from a movement along a production function, a movement which explains the output increase due to an increase in inputs and not to technical change. In our example the output change due to a shift in the production function is $q_2 - q_2'$, while the output increase due to a movement along the function is $q_2' - q_1$. The distinction between a shift

[2]*More precisely, the benefits of research, the implementation of new technologies, spread as sigmoid functions of time and space. The common assumption referred to in the text is, therefore, conservative in that it ignores all benefits which occur before full implementation of the new technology by all who will eventually adopt it. The diffusion literature focuses on rates of adoption and identification of critical points in the sigmoid diffusion functions. See Rogers (1983), the basic text in the field, and Buongiorno and Oliviera (1977), the only forestry article familiar to us.*

[3]*We could explain this process for either the consumers' and producers' surplus or the production function approach. We will explain the original production function/residual approach only as an example.*

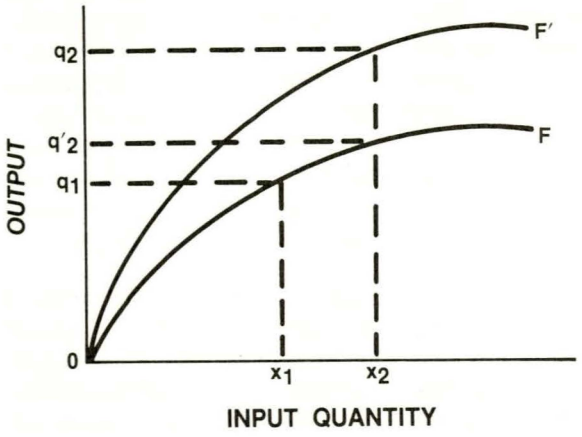

Figure 2.—*Production function.*

in the function and a movement along the function is difficult to make over time because input quality often changes, thereby contributing to and confusing measurement of any gross change in the production relationship. Accurate measures of technical change over time require inputs adjusted to reflect constant quality per unit.

The production function method for evaluating research has the advantage of permitting calculation of *marginal* research impacts. Knowledge of the functional dependence of F′ on its inputs, including its research and implementation related inputs, permits determination of the marginal physical product of any input. For example, consider the generalized form

$$F' = F'(R,K,L) \tag{1}$$

where R, K, and L are physical units of research, capital, and labor inputs, respectively. The marginal physical product of research is the first derivative of F′ with respect to changes in its physical research input—symbolically $\partial F'/\partial R$. The marginal value product of research MVP_R is simply the marginal physical product times the price of a unit of output P_Q:

$$MVP_R = P_Q[\partial F'/\partial R] \tag{2}$$

If the production function represents production in one year, then MVP_R is also measured annually. Once these benefits of research occur, however, they continue to occur year after year. Indeed, they form the basis from which future technical change occurs—and must be measured. Therefore, the marginal rate of return to research and implementation investments is that rate r which equates the discounted value of the perpetual stream of marginal research benefits with the cost of a unit of research related input p_R:

$$\int_{t=0}^{t} \tau MVP_{R(t)}(1 + r)^{-t}dt = p_R \tag{3}$$

where t = o is the year in which the research investment occurs.

One problem which arises is that capital and labor must be measured at some initial time, say t_o. Research must also be measured from some initial year, and therefore observed technical change is that which occurs between t_o and a later given year t_1. The earliest and most elementary empirical evaluations of research, whether using the consumers' and producers' surplus method or the production function method, calculate technical change as a residual. That is, they begin by estimating the functional relationship at the initial time t_o.[4] For the production function approach, this is

$$F(t_o) = F(K(t_o), L(t_o)). \tag{4}$$

The second step is to estimate the production function at a later given t_1, using the same functional form and new input levels $K(t_1)$ and $L(t_1)$, adjusted where necessary such that K and L refer to constant quality inputs:

$$F(t_1) = F(k(t_1), L(t_1)) \ . \tag{5}$$

Referring again to figure 2, this is the same as estimating the functional form F explaining the relationship between x_1 and q_1, and using this form to determine the output level q_2' which would occur with the new input level x_2. Technical change and the contribution of research inputs are assumed to be identical. They show up in neither estimate (4) nor (5), but are assumed to explain the full difference between the estimated $F(t_1)$ from equation (5) and the observed output level at time t_1. The observed level is one point (q_2, x_2) on F′ in figure 2.

METHODOLOGICAL PROBLEMS, RESOLUTIONS, AND EXTENSIONS

One difficulty that all empirical evaluations of research constantly struggle to overcome originates

[4]*Policies requiring "even-flow" of annual harvest volumes and restricting harvests to timber aged one-and-one-half times its biological maturity (itself usually twice the economic maturity) are examples. Others can be found in the expanding Wilderness System, in state forest practice acts, in high logging road standards, and in restrictions on herbicide and insecticide use.*

with inputs of varying quality. Increases in input quality can explain expanding levels even in the absence of technical change in the production process. Therefore, overlooking increases in input quality would bias upward any estimates of technical change or research impacts, yet the observed norm is for labor and processed capital inputs to do just that—to increase in quality over time as the labor force becomes better educated and as machines become more efficient. On the other hand, unprocessed capital inputs or natural resources may decrease in quality over time as society turns to lower grade and less accessible sources. Of course, decreasing quality of unprocessed capital and increasing labor quality have opposing biases on measured technical change. In sum, input quality is something to watch closely in empirical studies.

The obvious objection to the residual approach to estimating technical change outlined above is that the difference between $F'(t_1)$ and $F(t_1)$ includes both technical change and the net effect of errors in estimating the contribution of other, nonresearch-related productive inputs. Some have added that this difference also includes the impacts of scale economies in production.

Regarding scale economies, it may be true that over time the basic production operation becomes larger and that profits may result from this change in operational scale. That is, the increase in output is proportionally greater than the increase in inputs. This change in scale is not accidental, however. It must occur because of some additional knowledge about the production process—otherwise the old production operation would have been of the same larger scale. This new knowledge develops because someone carefully examined the production operation and learned a way in which the use of some basic inputs could be improved. This learning can be called research and the improved use of inputs is called technical change. Therefore, scale economies developing over time reflect technical change and shifts in production functions.

The objection that the residual between two production functions does not accurately measure technical change is valid, however. The residual calculated as we have discussed collects both a measure of technical change and the various errors attached to estimating impacts of the nonresearch inputs. The sum of these errors may be either positive or negative in sign and biases any estimate of technical change accordingly. The solution is to regress the observed difference between output levels in two periods on (a) the observed differences in constant quality labor and capital inputs, the (b) *research-related variables*, as well as on (c) any terms explaining interaction between research and other inputs. The resulting regression coefficients for the research-related independent variables then directly address the impacts of research. So long as there is no statistical collinearity between independent variables, then there is no longer any risk of collecting net estimation errors, attributing them to technical change, and identifying them as payoffs to research. The more recent economics literature makes this correction.

As empirical analyses of research impacts have become more sophisticated, they have broken the research term in their regressions ever more precisely into research itself and several related inputs. They may recognize both research and development, with development being the act of applying knowledge gained in research (Scherer 1980, p. 410). They may further divide research itself into basic and applied (Scherer 1980, p. 410) and into public and private (Cline 1975, Ruttan 1982), depending on the requirements of the particular empirical study. They may also recognize that education occurs as the handmaiden of research and hastens the pace of its implementation (Welch 1971), and that, for agriculture, extension activities play a large role in development. Finally, they may recognize that, over time, new political regulations on productions can interfere (Oster and Quigley 1977), constraining what could otherwise be sizable technical change and thereby constraining payoffs to research-related inputs. Not all of these distinctions in research-related inputs or constraints on their impacts are included as independent variables in empirical analysis, nor need they be. If they were all included, many would be statistically insignificant for any given product. It is important, however, to recognize these distinctions and to include the various research-related inputs where each is appropriate to the production process and to the objective of the particular analysis.

Questions of which research-related inputs to include in the regressions assessing technical change raise similar questions regarding cost accounting. The full cost of technical change includes the entire sequence of costs, from those associated with basic research through those associated with applied research to development costs. In application this often means public expenditure for basic research, private expenditure for modifications to satisfy specific needs, and private expenditure for development, which suggests modification or exchange of existing equipment and retraining labor.

The general experience of the U.S. Forest Service's Forest Products Laboratory provides a useful example. The Forest Products Laboratory generally undertakes risky and long-term basic research projects having to do with new product development or potential improvements in mill operations. Forest

products industrial firms generally begin to show interest when the Lab shows initial signs of research success. By then both the long time period between initial research and final application and the riskiness of research success have been reduced. Subsequently, the research components of industrial firms take the public information gained from the Lab's basic research experience and begin a race with each other and the Lab to develop the applied patents. Finally, the firms make the necessary modifications in their mills to implement the newly patented production process or to produce the newly patented product.

In other cases more common to the experience of agriculture and relevant to timber production by nonindustrial private landowners, the public sector is the source of funds for both basic and applied research. The public sector is also the source of agricultural extension funds with their educational emphasis. Of course, there may be varying Federal and State shares, with the States often becoming relatively more involved with extension. After receiving new production information from extension agents, nonindustrial private landowners then modify their newly acquired knowledge to fit their own objectives and their existing operations.

The cost of technical change is the cost of all of these research-related operations. If we choose to examine, say, Federal research expenditures, then we must recognize that we are only examining one component of the total costs of technical change. We must compare the costs of this component with only their appropriate share of total technical change, a share which can be found using proper regression techniques and two research-related independent variables, one for Federal research expenditures and the other for all other research and development. This technique avoids the temptation to count all of a given technical change as the benefit resulting from a single component of all costs, which, in this example, would unjustly ignore the efficiency of State and private research-related expenditures and inflate the efficiency of Federal expenditures. It is not unreasonable, however, to compare each independent research or development cost with its own share of the benefits of the given technical change. When doing so we must just be careful to not overlook any research and development inputs and to avoid double-counting any benefits of technical change.

We have recognized a sequence to the benefits and costs of technical change. This raises questions of the time lags between initial basic research and final implementation, and of the diffusion of successful technical change over time and space. The answers to these empirical questions vary from case to case. Nevertheless, time lags are important because only a few years' delay in implementation can substantially reduce the discounted benefits of a given technical change. We might anticipate that lags between research and implementation are shorter in concentrated industries where a few single firms have large stakes in the gains from a new technology. Therefore, we anticipate shorter lags in the relatively more concentrated pulp and paper industry than in the lumber industry, which is composed of many small sawmills (and a few larger ones). We also anticipate shorter lags in wood-utilizing industries in general than in the timber-growing industry, where much of the production occurs on small ownerships of nonindustrial forestlands. Our anticipated empirical analyses of different wood product industries will provide tests for these hypotheses.

Finally, we should introduce the related idea of technology assessment—although technology assessment *per se* is not our objective in this paper. Producing technology assessments has itself become a large activity for researchers and consultants in recent years. The Congressional Office of Technology Assessment has been particularly active, but U.S. Department of Agriculture agencies have also requested such assessments from time to time. The purpose of technology assessments is to show the impacts of a given production technology on energy, employment, environment, or whatever social, economic, and political decision criteria may be important to local communities affected by location and use of the technology in question. Because of the current popularity of technology assessments, it may be useful to comment on their relation to research evaluations like ours. The relationship is easily shown with reference to our figures 1 and 2.

Technology assessments measure the impact of a given technological change—often an anticipated change, not an actual one—shown by a shift in the supply function from S to S′ and a shift in market equilibrium from a to b in figure 1, or a shift in the production function from F to F′ in figure 2. Technology assessments require a measure of the gain displayed by these shifts, which is related to the benefits in our research evaluations. But when technology assessments ask about energy, employment, environment, or whatever, they are asking about changing use levels in the factors of production. Supply and production functions measure the impacts of the aggregate of all productive factors. Therefore, to answer these questions is conceptually simple. We need only to examine the component parts of these functions or, equivalently, the capital and labor inputs in equations (1) through (5). Of course, the empirical difficulties are that these inputs may have to be more precisely specified than

just "capital" or "labor." The specification may be particularly troublesome if (1) the distribution of a given input among affected populations is important or (2) some of the inputs are not readily measurable or, even if measurable, are treated as open access resources (as environmental inputs like air and water quality are often treated).

AN HISTORICAL PERSPECTIVE ON TECHNICAL CHANGE IN AMERICAN FORESTRY

We have commented on the disproportionate impact which others have found that research has on economic growth and we have reviewed the basic analytical methods which they used to derive their results. The methods have improved with practice. The validity of the results is established and the results themselves remain impressive. This encourages us to proceed with our specific inquiry into forestry research. Has its impact been as impressive? Before we begin with our empirical estimates, however, let us also review the history of forestry in the United States. Understanding this history may help us anticipate the locus of successful forestry research in the past, as well as some of its future characteristics.

Until the last 35 years, American forestry could be characterized by the frontier model of economic development. That is, land was relatively more plentiful than labor as a factor of timber production and transportation costs were not a substantial deterrent to harvesting. Growth in output was accounted for primarily by increased harvest area. Furthermore, the conservation model of forest development, emphasizing notions of crop husbandry and soil depletion, never fully transferred from Europe to the United States. In an economic environment characterized by high and rapidly rising wage rates, advances in mechanical technology, particularly in timber harvest and utilization, dominated the more labor-intensive conservation practices such as thinning and pruning. This describes our forest economy (only fire control may have been an exception) since the Jamestown settlement. American forestry is only now beginning to change from a frontier economy to a developed economy, where land is also a scarce factor of production.

This history is most easily seen by following the geographic expansion of forestry across the country. North America presented its European colonists with a natural endowment of mature timber. Harvest of this timber from the time of colonization through the mid-19th century concentrated in New England. The Great Lakes States' pinery was harvested in the late 19th century. There was considerable harvesting in the South throughout this time, but the South was never the dominant timber production region. In the early 20th century, timber harvesting moved to the West Coast, where the trees were big and water-borne transportation was readily available. It gradually moved down the coast from Seattle and up the coast from San Francisco. The Rocky Mountain and Intermountain West were skipped over because the timber stocking was less dense, access was more difficult, and transportation costs to the large eastern markets were great. Harvests in these regions are still not large, although they have become somewhat more important since World War II.

These geographic trends are a generalization, of course. There has always been some harvesting for local use within each region. Even within regions, however, harvest has generally followed the frontier model; from more to less accessible land, from better to poorer quality species, from better to poorer stocked timberlands, and from higher to lower grade trees.

Timber production has been characterized by *harvesting* throughout the 375 years of European development of North America. There has been little incentive to spend scarce resources *growing* timber while mature timber remains. The mature timber, however, is about gone and the period characterized by concentration on harvest technologies is drawing to a close. This event is, perhaps, hastened by public agency policy and recent environmental restrictions on timber harvesting and land use, restrictions which generally make it more difficult to harvest the remaining mature timber on public lands. In other words, the timber frontier is closing, timberland is becoming relatively scarce (more valuable), and the attention of timber producers is turning to plantation management (timber growing). Nowhere is this more important than in the South, which is becoming the leading timber-producing region in the world.

The long-term upward relative price trends of stumpage and saw logs support this view (Ruttan and Callaham 1962, Barnett and Morse 1963, Phelps 1977, Manthy 1978). These trends might be expected to continue as long as a stock of mature timber remains. Indeed, the rate of relative stumpage price increase might be expected to reflect the rate of depletion of the stock of mature timber. Berck (1978), Libecap and Johnson (1978), and Lyon (1981), in independent analyses, report empirical results consistent with this hypothesis.

Rising relative stumpage prices are entirely consistent with the expectations of the economic theory of stock resources—which is best known for its application to hardrock mineral extraction. The standing mature timber available for the early colonists was like a newly exposed mineral deposit which comprises a large share of all of that particular mineral available at some moment in time. Availability of this new deposit sharply and immediately depresses the market price of the mineral. As the new mine is gradually depleted, only deeper and lower-quality material remains. The mine's production costs increase. Eventually, the mine loses its competitive production cost advantage over other mines, and the market, including production from all other deposits of this mineral, stabilizes at some new natural price level.

Similarly, one expects stumpage prices to increase as long as there is a stock of mature timber and as harvest of this stock moves to less accessible sites, less densely stocked stands, poorer quality species, etc. The rate of price increase will eventually taper off and stumpage prices will find their natural level when all the mature timber is gone and the costs of growing timber equal the value of timber produced. The small remaining volume of mature timber in this country and the financial successes of southern timber plantation management suggest that this will occur soon. Berck (1978) and Adams and Haynes (1980) project that it will occur within the next 30 to 50 years. In other words, they expect constant relative stumpage prices within the next timber rotation.

The important questions for us are what this history suggests for technical change in the timber and wood products industries. Because technical change is the product of research, a review of harvesting history allows us to hypothesize where research has been productive and to anticipate where it may be productive in the future.

There was little incentive to develop land-saving technologies when land was relatively more plentiful than labor, and where harvests originated from a ready inventory of mature timber. Timber production per acre actually decreased in the first half of the 20th century—although aggregate output expanded during this time. (Output per acre decreased as stand density, access, species quality, etc. declined with the expanding frontier.) Incentives favored labor-saving technologies, which were *mechanical* (as opposed to biological) and which concentrated on wood utilization and processing (as opposed to timber-growing) activities. In the future, as land becomes relatively scarcer, this picture may change considerably. There may be an expanding search for ways to increase production per acre through labor-saving technical change probably of a more biological orientation.

The available evidence supports these hypotheses. The well-known advances in wood utilization and processing are numerous:

1. Introduction of steam-powered sawmills and steam-powered locomotives in harvesting (c. 1870).
2. Introduction of the band saw, reducing loss to sawdust (c. 1870).
3. Introduction of gasoline- and diesel-powered mills and woods machinery (c. 1910, exclusive use by 1930).
4. Introduction of power saws for felling and bucking (c. 1920).
5. Introduction of the sulphate pulping process, permitting the use of resinous southern pines (c. 1920).
6. Introduction of wheeled and crawler tractors for skidding (c. 1930).
7. Sawmill electrification (c. 1932).
8. Introduction of the semichemical pulping process, permitting use of hardwoods (c. 1940).
9. Development of the chip-n-saw, permitting expanded use of sawmill waste in pulping (c. 1962).
10. Development of improved lathes for plywood and veneer manufacture, contributing to the opening of the southern pine plywood industry (c. 1962).
11. Introduction of computerized scanning in sawmills, improving log utilization (c. 1970).
12. Decreasing standards in dimension lumber (2×4's are now actually $1\ 1/2 \times 3\ 1/2$ inches).
13. Increasing mill specialization, permitting decreased standards in harvested logs. (Acceptable logs in the West were 32 feet in length, they are now 16 feet. Eastern standards have decreased from 16 to 8 feet.)

While biological advances such as thinning, fertilizing, and various improvements on natural regeneration were known, they were applied only rarely and haphazardly until recently. Forest fire control may have been the exception. Certainly it has consumed major public effort since the 1930's, but the impact of this effort is difficult to demonstrate. We anticipate discussing it further in an empirical investigation of the research-fueled gains in timber production.

The few quantitative analyses support these hypotheses. Horsepower per woodsworker has increased steadily since 1919 and the capital-labor ratio has similarly increased—at a rate more rapid than that in either paper or metal products. Nevertheless, a decline in land productivity from 1910

until 1950 offset it—causing a decline in labor productivity until 1940 (Ruttan and Callaham 1962). Perhaps the decade of the 1940's marks the beginning of a change from a frontier to a developed timber economy. The value-added due to lumber processing continues to be relatively small and the rates of both productivity growth and technical progress in the lumber industry are approximately half of those for all industries for the United States in the 20th century (Kendrick 1961, Stier 1980). These rates have increased only somewhat since 1949 (Robinson 1975). Value-added by processing activities in the pulp and paper industry is much larger and the rates of both production and technical progress in this industry are more commensurate with those in other manufacturing industries. Moreover, there has been a bias toward labor-saving technical change in all wood products industries, but particularly in the sawmill and planning industry (Hunter 1955, Holland 1960, Stier 1980).

We might anticipate that these trends will continue a while longer, but without their previous strength. Forestry in general, and southern forestry in particular, is converting to more land-saving timber-growing technologies. While biological research probably has not been highly productive, it may become more so. If we find that the economic returns on biological research have been low, then we might expect to find only a small amount of private research investment. In the past, returns have been too uncertain and too distant to attract private investors. The many small timber producers may have also found investments in tissue culture and thinning regimes, for example, a severe strain on their limited financial resources. Furthermore, it is difficult to establish proprietary claims to the return on these research investments. These are all reasons why, in the past, public agencies may have been responsible for the largest share of biological forestry research. While private biological research can be expected to expand in the future, these are also reasons why public research will continue to be important.

There may be considerable transfer of new biological technologies from American agriculture, where there is more experience with biological growth functions and from European forestry, where land has traditionally been a scarcer factor in timber production. Moreover, as the leading timber-producing region, the South may expect its biological or scientific research to produce benefits which eventually spill over to all other timber-producing regions of the country. Research directed toward utilization and process technologies will continue to be attractive, as will labor-saving innovations in general. Biological research and technical change, however, may become more important components of the total forestry research effort.

ORGANIZATION OF THE PROJECT

The empirical analyses which we have begun will provide evidence for the various hypotheses raised in the historical review. We plan to set the scene for these analyses with an introduction to the institutional arrangements for funding forestry research. The main theme of the empirical analyses themselves is the effectiveness of forestry research expenditures. We attempt to calculate the economic return to dollars spent in research. We also comment on distributional impacts of research expenditures where the data and analytical method permit—focusing on distributional impacts among factors, landowner classes, geographic regions and consumers.

Our general objectives are twofold: To demonstrate the methodology as it necessarily changes for various analytical cases, and to derive policy-oriented conclusions. The convenient methodological and policy divisions are between aggregate or forestry-wide studies and industry-specific (e.g., sawmills, plywood and veneer mills, wood preserving, pulp mills, etc.) studies where analysis of research impacts are of either an *ex post* or *ex ante* nature.

	ex post	ex ante
aggregate processing	X	—
biological	X	—
industry-specific processing	X	X
biological	X	X

Ex post analyses provide critical review of previous investments in forestry research and should be conducted with the expectation of learning from previous experience. *Ex ante* analyses anticipate the benefits and costs of forthcoming research investment alternatives. They are conducted with an eye on the planning process.

Our aggregate analysis provides comment on the overall success level of previous (*ex post*) investments in forestry research. The data are such that these aggregates are best divided into four industrial subcategories corresponding to two-digit Standard Industrial Classifications (08, 24, 25, and 26, or timber growing, lumber and wood products, furniture, and pulp and paper, respectively). It is important that investigations into the first, timber growing, began early in the 20th century and include fire control research expenditures made since the 1930's. Productivity data are weak this far back, however.

Therefore, this analysis can be conducted only with some skepticism. The research contributions of the remaining three SIC code industries are more recent and easier to follow. The timber growing aggregate should shed some light on the previous general productivity of biological forestry research. The other three aggregates should provide similar insight into research leading to mechanical or timber-processing innovations.

If the aggregate analysis shows general and broad industry-wide impacts of research, then it remains for us to examine more closely the impacts of more narrowly defined and explicit research expenditures. This is the intent of three other components of our project. It is difficult to separate the impacts of explicit research projects when research may proceed simultaneously on several distinct projects within a given industry. A reasonable solution is to define the industries as narrowly as the data permit, thereby restricting the likelihood of measuring benefits to multiple research projects. Therefore, in these project components, we examine the softwood plywood industry and five four-digit SIC code industries. The presentation of our method of analysis is detailed and complete for the softwood plywood component in the hopes that this discussion may serve as a useful model for both the remainder of our project and for future work by others. There were no southern plywood mills prior to 1962, yet the South has the dominant place in the industry today. The contribution of research to southern plywood development is an important additional issue in this chapter. A second project component reviews more briefly research impacts in the five four-digit industries: sawmills, structural wood members, wood preserving, pulp mills, and paper mills. Our concentration in this component is on results, not method. The final of these three components applies the results from the previous two to ask the Schumpeterian question about research and firm size: Do firms of a larger or smaller absolute size expend greater research effort? Or does size have little to do with research expenditure while market share may be a better predictive variable? (Schumpeter 1950, chapter 2; Scherer 1980, chapter 15). Our observations from this investigation may be suggestive of which forest product industries are most likely to conduct their own (private) research and which may benefit most from greater public support and encouragement.

These are all *ex post* analyses. They can tell us what has been and perhaps throw light on the characteristics of previous research successes and failures. Another major unit of our project examines loblolly pine research, including the most important genetics research in forestry. This unit is new in its explicit biological research focus. Some loblolly pine research is really quite old, but many impacts of the research are not known with certainty. Most biological research impacts can only be anticipated today, since they are, at best, represented by standing plantations which have yet to mature and be harvested. Therefore, this unit of our project must anticipate *future* benefits to current and past research. The results of this analysis may shed light on our previous hypothesis that biological research investments in forestry will become more important as the forest economy matures in the latter part of the 20th century.

There is no need for an aggregate and *ex ante* analysis because aggregate planning decisions are generally based on either past reputation—as tested here in the aggregate and *ex post* case—or the sum of expected good decisions on specific current and future research projects. The *ex ante* analysis of loblolly pine research investments is an example of the latter.

Finally, we intend to summarize our findings and draw conclusions. They should have policy implications to research planning and expenditure in both public and private forestry. In particular, these findings should call on our general insights from the previous empirical analysis to suggest the characteristics of successful research projects, their impacts on local economies, and the justifications for both public and private research activities.

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DISCUSSION IV

DAVID LEWIS

To what degree do you see that the evaluation of any specific technology is highly conditional and the results are really dependent upon a whole range of technologies? I am not really sure that we can separate technologies, and in order for technology to have an impact, whatever it is, it has really got to be part of a whole sequence of accumulated knowledge. You can't have genetic improvement without some pretty fair nursery practices and also some stand management practices to capitalize on the improvements. What have you found?

HANS GREGERSEN

I don't think there is any question that everyone has that problem with evaluation. You have that problem with almost any kind of evaluation whether you are looking at research or anything else. This is the problem of accounting for all the costs. Obviously it depends on the particular thing you're looking at. The more detailed you try to get, the more you run into that problem. One way to get around that is to look at whole program areas.

BILL HYDE

I certainly agree with what Hans said about the more detail you get into, the more you have that problem, but it's for that reason you look at broader categories. That, incidentally, follows the idea of externalities in environmental economics. The broader you define the geographical area the better you can incorporate your externalities, therefore the fewer the problems you overlook. A second point has to do with the whole idea of questioning what has been the overlap, what has been gained from other industries. There are some ways of getting at least a handle on the spillover effects. In fact, some of the agricultural literature actually tries to measure spillovers. The third point is that you can regress outputs on expenditures and while significant correlations do not prove causation, they do lend confidence to your hypotheses.

HANS GREGERSEN

One of the intriguing questions they tackled in agriculture evaluation, to some extent, is the time length between technologies. For example, in crop improvement research, what do you do about the guy 10 years ago who fed into the ideas of research today? Similarly, if you look into the future, when do you cut off the benefits? I think agricultural research evaluations have typically taken a kind of pyramid approach with the benefits going up, and then down. I think it is an arbitrary thing in a sense, as to where you cut off the benefits of new technology and how you treat costs of the old.

BURT SUNDQUIST

Whenever you get into economic research you are faced with laying out good research, whether it be evaluation research or some other. It's dependent upon being able to "partialize" a meaningful set of events or situations. By that I mean, in the real world, at one extreme everything is related to everything else in some degree, and at the other end of the spectrum you have individual events. Evaluation at either one of these extremes typically is not very useful. So then the question is where in this spectrum can one meaningfully partialize in order to do research that corresponds to decision points, or in this case, allocation of research money, or what have you. In the final analysis, one has to be pretty pragmatic, and say it is meaningful to try to generalize from this part of the spectrum—cutting it off here—even though you realize that it's not an entirely isolated situation. One of the criteria for doing that should be, "If I can partialize at this level, does this correspond with a meaningful decision point? Does this evaluation of the subaggregate, or this situation, have some meaning in terms of somebody being able to make decisions or change their behavior on the basis of this information?"

JOHN FEDKIW

As for concentrating on who needs evaluation, largely for the budgeting and allocation question, I think that the implications of research and evaluation, both ex post *and* ex ante, *have far broader applications than that. As we accumulate this understanding of what research does to forest productivity, both in terms of total productivity and the input productivity itself, our analyses will reveal strategies which may tell us where future forest production may go in a technological sense. An outstanding example of research of that sort is sycamore silage, which was a revolutionary idea. And of course all the chipping and the fiber approaches break down the dimensional limitations of trees. This has tremendous strategic significance for research planning and forest management. Also important is raising the sensitivity of managers to what technology can do to address both problems and opportunities in resource management.*

Sensitivity to the potential of technology can do a great deal to accelerate the rate of adoption of research. Finally, there is the strategic value evaluative information to States like Minnesota and Michigan, which are looking for more job opportunities. We know from history that this type of information gives support and justification for taking initiatives, as they are doing. Industry itself is very aggressive toward the potential of new technology because it is seeking opportunity. I think we ought to take a broader look at the value of information from ex post *and* ex ante *evaluations beyond the budgeting of Federal and State programs.*

BILL HYDE

In any two industries you choose, regardless of whether they are subcomponents of forestry or of agriculture, there would be some important differences. For example, agricultural cases differ from the softwood plywood case in that the extension is important for disseminating new agricultural research results. There can be long time lags between research and implementation in agriculture. The diffusion rate and the cost of getting new information out to many farmers can be critical. In softwood plywood, however, the industry has somebody at the Forest Products Lab looking over the shoulder of the engineer who is doing the work, and there is no time lag, and no diffusion problem at all.

HANS GREGERSEN

The top eight forest products companies together have something like 80 percent of the expenditures in private forest products research. Most of the southern plywood research is private money, so it is a fairly concentrated area.

JOHN FEDKIW

For aggregate evaluation it may be easier to go at it by segments rather than comprehensively, for example: focus on what research may have done to reduce the cost of reforestation. Technology of this sort raises rates of return. Reduction in total costs of inputs in terms of unit cost per cubic foot of final product harvested or something of that sort is another important segment of the aggregate impacts of research.

It's important because the biggest cost in timber production is the cost of carrying capital. Anything that raises the rate of return because of reducing initial costs will influence the incentive that people may have to invest to expand supply.

ROBERT BUCKMAN

I wanted to comment on a couple of the technologies that have been discussed here, and I don't quite know how these observations fit in with research evaluation, but maybe they do. First, on the power backup roll. Now my guess is that we handle the benefits of the power backup roll by reductions in spin-out, the fact that we are able to put logs in a lathe that happen to be soft at the core. But I have the impression that the power backup roll may spawn a second generation of industries that we have not given much credit to. It may be possible for us to rotary-peel wood that we have not peeled before and to peel it a lot thicker than we ever have before. This raises the possibility of laying up those thick slices in parallel lamination and some of that work has already been done. But then it spawns still another possibility: On difficult-to-treat species we have lathe checks because of the thick slicing, and we could drive preservative right into the center of wood, something we couldn't do before. It is kind of an interesting second- and third-generation technology that may come out of something that was intended to reduce spin-out in a mill.

The second technology is one that is related to sycamore silage. Back about 15 years ago, the American Pulpwood Association came onto the notion that if we could separate bark from chips, we could make a major breakthrough in the recovery of pulpable species. And the American Pulpwood Association lobbied our Appropriations Committee and got funds for that activity. John Erickson and his colleagues set standards, in consultation with industry, for the amount of separation that we had to get in order to run both the chips into a pulp process. We achieved those standards. As I recall, some species were easier to work with than others. It was possible for us to chip whole trees and separate the bark from the chips. In the interim, pulp prices went up and the pulp and paper chemists discovered that they could run a lot more bark through their digesters than they thought. And so here was a fascinating technology that offered the opportunity to increase the per acre returns something like 50 percent.

The research was highly successful. It had a strong user identification and yet as nearly as I can tell it is not being used to date. I think there is kind of an interesting question about a major technology with enormous resource implications that is simply not being used. Now I don't know how we deal with that. If we are looking for something lying dormant or that is a failure maybe here is a case in point. I wonder if it's one of those technologies that is just going to lie dormant for a little while, and all of a sudden we will design a new mill to use it.

DAVID LEWIS

We looked at it very closely and it is a very interesting thing because it is quite dynamic and very much driven by the price system. At an integrated facility you've got some marginal material that can easily be identified as dirty chips. Whether it goes into fuel or into a fiber product depends in part on the price of fuel and in part on the price of composite panels. Building a facility that has the capacity to take advantage of both possibilities is the way we did it.

JOHN FEDKIW

That opens up a related question in another area of economics. What is the economics of industrial innovation; not only in general but within an industry, between industries, and between sizes of firms? We had some very fascinating demonstrations of how that works. When stumpage supplies became limited in the Douglas-fir subregion in the 1950's and 1960's, plywood capacity expanded while timber production declined. The new technology of plywood for housing applications and related prices of each products and profits, made it possible for plywood producers to outbid lumber mills for the available stumpage as little innovation occurred. Timber supplies in the West continued to dry up and become scarcer and higher priced, and we had a great deal of acceleration of improvement in mill technology. Some of it came about because of research and technology transfer on sawing methods. If we hadn't had the scarcity of supply developing in the West, mill improvements wouldn't have come nearly as fast as they did. In the housing industry, because of a high rise in lumber cost and interest rates there have been efforts to find ways to cut back costs of new homes. Truss-frame construction reduces wood required by 30 percent and provides the space enclosure that one wants in housing. The adoption of truss-frame construction is increasing steadily. There are periods when some industries are very aggressive toward new technology, and other times when profits are great and they are not interested. They aren't going to replace their old capital with new, more expensive capital as long as they're operating profitably.

AN OVERVIEW OF THE REGIONAL FOREST NUTRITION RESEARCH PROJECT

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The Regional Forest Nutrition Research Project (RFNRP) is an industry-agency-university research cooperative established in 1969 at the University of Washington. The principal objective of the cooperative is to provide forest resource managers with accurate data on effects of fertilizing and thinning young Douglas-fir and western hemlock stands in the Pacific Northwest. To achieve its objective, the cooperative has established an extensive network of permanent growth plots. These plots were located according to an experimental design which permitted regional as well as subregional analyses to be conducted. Results have been widely distributed and the project, now in its 15th year, is being extended for another 5 years.

In October, 1982, the U.S. Forest Service funded a 2-year project at the University of Washington. The objectives were to describe the inputs, outputs, application of results, and the impacts of the RFNRP on society. This case history collates all available information relative to the cooperative including: (a) the history of the RFNRP from its inception to the present; (b) the importance of strong leadership in founding and guiding the cooperative over time; (c) the total dollar investment; (d) the allocation of the total investment to salaries, supplies, travel, etc.; (e) the number of staff and graduate students supported; (f) the number of plots established; (g) publications produced by the project; (h) the principal research findings of the cooperative and their societal impacts; and (i) a benefit cost analysis of the cooperative. The purpose of this paper is to briefly review the findings of our investigation to this point. Full details of the case history may be found in a separate report (Bare and Loveless 1985).

HISTORY OF THE RFNRP

The origins of the RFNRP can be traced to discussions held during the 1960's by the Pacific Northwest Forest Soils Council. The Council, active since 1947, provided a convenient vehicle for information exchange between forest industry, university, and public agency scientists and land managers. One of the problems discussed by the Council concerned the apparent nitrogen deficiency of Northwest forest soils and its subsequent effects on site productivity. This problem was brought to the attention of the forestry community by the pioneering work of Dr. Stanley Gessel of the University of Washington, who had completed nearly 20 years of research into this problem by the mid-1960's.

In 1967, a Tree Nutrition Committee was formed by the Council to consider ways to coordinate further research into the soil nutrition problem. Even with the significant work of Dr. Gessel, research results concerning nitrogen fertilization were fragmentary and limited in scope. Consequently, no operational fertilizations were being conducted in the 1950's and early 1960's. Further, results of research fertilization trials were often inconclusive—good response to fertilization was observed on certain sites, with little (if any) response elsewhere. Recognizing this diversity and the large geographical area requiring study, the Committee suggested that a cooperative be initiated in the Pacific Northwest.

Because of Dr. Gessel's research record in this area, and his involvement on the Council, he took an active role in the formative stages of the new cooperative. Furthermore, the cooperators recognized that an institutional home with good relations among forest industry, public agencies, and academic institutions was a prerequisite for success. Historically, the USDA Forest Service has provided homes for cooperatives with regional significance. However, in this instance, the decision was made to house the cooperative at a university where institutional stability could be expected over an extended period. Consequently, the Committee, working with potential cooperators, decided to house the cooperative at the University of Washington.

Research proposals were circulated by the Committee in July 1968 and the cooperative was ini-

tiated in 1969. It's main objective was to develop information on response to nitrogen fertilizer of Douglas-fir and western hemlock over a wide range of young stands so that sound decisions could be made regarding forest fertilization. Although the original intent was to simultaneously examine the consequences of both fertilization and thinning, first-year funds were only sufficient to initiate fertilization trials.

Starting in 1969, a set of permanent plots was established in unmanaged, even-aged, fully stocked stands throughout the Douglas-fir region of western Washington and Oregon. Known as Phase I, this portion of the study involved the establishment of 85 Douglas-fir and 32 western hemlock installations in six Douglas-fir and three western hemlock provinces.[1] Each installation consisted of six plots (two control, two fertilized with 200 lb of nitrogen as urea per acre, and two fertilized with 400 lb of nitrogen as urea per acre). All plots were at least 1/10 acre in size. Plots established in 1969-1970 currently have 14 years of growth measurements.

In 1971, when additional funds became available, a second set of installations known as Phase II was established. These installations included six plots which had up to 40 percent of their basal area removed at time of establishment. Subsequently, two of the plots were fertilized with 200 lb of nitrogen as urea per acre and two were fertilized with 400 lb of nitrogen as urea per acre. A total of 35 Douglas-fir and 8 western hemlock installations was established in Phase II. Plots established in 1971-1972 currently have 12 years of growth measurements.

In 1975, Phase III of the RFNRP was initiated. The objectives of this portion of the study were to establish additional installations on poor sites and in young stands (10-20 years old) to strengthen the Phase I and II databases. Most of the young stands were precommercially thinned prior to fertilization. Twenty-three new installations were established in Douglas-fir and six new installations were established in western hemlock during Phase III. Further, one-half of the Douglas-fir Phase I and II plots were refertilized after the eighth growing season. Plots established under Phase III currently have 8 years of growth measurements.

In 1980, Phase IV of the RFNRP began. In addition to locating 14 new Douglas-fir and 4 western hemlock installations in young (8-15 years old), precommercially thinned plantations, an additional 12 Douglas-fir installations were located in poorly stocked stands. Last, four western hemlock installations were located to balance the Phase II design. Some of these latter installations had 60 percent of their basal area removed, while others had 40 percent removed. In Phase IV, the cooperative also: (a) refertilized half of the Phase I western hemlock plots originally fertilized 10 years earlier, (b) refertilized all of the Phase I Douglas-fir plots which had received a second fertilization 4 years earlier, and (c) refertilized half of the Phase II Douglas-fir and western hemlock plots. Phase IV of the RFNRP is to be completed by June 30, 1985, at which time Phase V will commence.

OBJECTIVES OF THE RFNRP

As previously stated, the primary goal of the RFNRP is to develop information to guide the planning of large-scale fertilizer applications to forest lands in the Pacific Northwest. More specific statements of objectives can be found in a variety of biennial reports, technical notes, and project proposals. Recently, Hazard and Peterson (1984) reviewed these goal statements and concluded that none of the earlier documents sufficiently states project objectives to permit the formal testing of hypotheses. They propose that the testable hypotheses underlying the RFNRP are: (a) whether fertilizer response differs among provinces; (b) whether fertilizer response differs between thinned and unthinned stands, and interacts with province; (c) whether fertilizer response differs between levels of fertilizer application, and interacts with level of thinning and province. They also propose development of predictive models for fertilizer response as a function of province, level of thinning, level of fertilizer application, interaction effects, and significant stand variables. After 15 years of plot establishment and remeasurement, many types of analysis have been conducted in an effort to provide answers to the questions inherent in these four objectives. In this report, only a brief summary of these findings will be presented. More detailed results can be found in Bare and Loveless (1985), and RFNRP biennial reports.

INVESTMENT IN THE RFNRP

The RFNRP has established about 2,000 permanent growth plots at a total direct cost of $2.5 million. Table 1 shows the annual spending pattern through FY 1984. Twenty-two cooperators have financed the cooperative on a continuing basis. Cooperators include representatives from: (a) chemical and fertilizer companies, (b) forest industry, (c) State forestry agencies, (d) Federal forestry organizations, and (e) Indian nations. Approximately 71 percent of

[1] *A definition of these provinces is contained in a later section of this paper entitled "Province Analysis."*

Table 1.--Summary of expenditures for all phases from project initiation to June 30, 1984.

(Nominal dollars)

Fiscal year	Salaries benefits, overhead	Travel	Supplies equipment, contractual services	Annual total	Cumulative total
69-70	$ 57,296.52	$ 17,179.92	$ 10,580.53	$ 85,056.77	$ 85,056.77
70-71	77,052.25	18,052.08	5,663.31	100,767.64	185,824.41
71-72	69,875.49	12,680.81	10,339.28	92,895.58	278,719.99
72-73	68,945.49	14,581.74	9,016.18	92,543.41	371,263.40
73-74	67,002.83	19,731.71	9,628.02	96,362.56	467,625.96
74-75	122,735.47	16,123.71	15,951.16	154,810.34	622,436.30
75-76	114,786.86	23,355.79	16,957.83	155,100.48	777,536.78
76-77	69,623.56	19,154.61	22,834.42	111,621.59	889,158.37
77-78	124,438.34	33,071.33	16,997.31	174,506.98	1,063,665.35
78-79	135,525.93	34,826.34	25,437.35	195,789.62	1,259,454.97
79-80	151,065.41	31,614.70	40,649.71	223,329.82	1,482,784.79
80-81	175,476.08	40,136.30	26,400.54	242,012.92	1,724,797.71
81-82	140,997.77	27,673.79	25,334.44	194,006.00	1,918,803.71
82-83	156,981.40	32,209.22	39,473.26	228,663.88	2,147,467.59
83-84	200,482.00	45,274.00	61,271.00	307,027.00	
Total	$1,732,331.00	$385,660.00	$336,534.00		$2,454,495.00

the total investment has been contributed by industry; 9 percent by the States of Oregon and Washington; and 20 percent by the Federal government and Indian nations.

Indirect costs of the cooperative not reflected in the $2.5 million of direct costs include: (a) the cost of fertilizer donated by chemical companies; (b) the contribution of university faculty, agency personnel and scientists, and industrial scientists who contributed their expertise in an advisory capacity; and (c) the time spent by cooperators in locating suitable sites for plot installations.

Approximately 71 percent of the direct costs have been expended on salaries, benefits, and overhead for project staff and graduate students; 16 percent on travel; and 13 percent on supplies, equipment, and contractual services. The cooperative has supported 2 to 5 graduate students (half-time), and 6 to 14 staff (3 to 5 full-time) per year. The project's field crew accounted for almost all of the travel expenditures and 60-75 percent of the salaries.

MAJOR RESEARCH RESULTS

RFNRP biennial reports contain an accurate and detailed chronology of the results achieved by the cooperative. Apart from a variety of special studies discussed below, the greater part of each biennial report has been devoted to a discussion of research results relating to the first three hypotheses proposed by Hazard and Peterson (1984). Thus, it is appropriate to begin this section of the paper with a brief description of these findings.

Unthinned Douglas-fir

The 10-year periodic annual increment (p.a.i.) responses to one fertilizer application as expressed in total gross cubic-foot volume/acre/year (CF/A/Y) are: (a) 40 ± 10 CF/A/Y for 200 lb of nitrogen per acre and (b) 50 ± 10 CF/A/Y for 400 lb of nitrogen per acre. This response is not significantly related to stand age, site index, or stocking variables. However, there is a general trend of increasing response as site index decreases.

Total gross CF volume p.a.i. response declines with time after treatment, and is no longer significant after the 10th year. This duration of response is longer in stands of low site index and is greater with the higher rate (400 lb per acre) of nitrogen application.

The 8-year p.a.i. responses for one fertilization as expressed in merchantable gross CF volume are: (a) 35 CF/A/Y for 200 lb of nitrogen per acre, and (b) 49 CF/A/Y for 400 lb of nitrogen per acre. Again, volume response is not significantly related to site index, stand age, or stocking variables.

Thinned Douglas-fir

The 8-year p.a.i. responses for one fertilizer application as expressed in total gross CF volume are: (a) 55 ± 6 CF/A/Y for 200 lb of nitrogen per acre and (b) 66 ± 6 CF/A/Y for 400 lb of nitrogen per acre. Again, there is no evidence that volume response in thinned stands interacts with stand age, site index, or stocking variables.

Total gross CF volume p.a.i. response declines 4 years after treatment, although the response is still significant 8 years after fertilization.

Thinned and Unthinned Western Hemlock

Neither the thinned nor the unthinned western hemlock installations have shown a significant response to the application of either 200 or 400 lb of nitrogen per acre. The only significant response for total gross CF volume has occurred in thinned young stands in one province.

Province Analysis

As previously stated, one of the objectives of the RFNRP is to test whether fertilizer response differs among provinces. Accordingly, six Douglas-fir and three western hemlock provinces located in western Oregon and Washington were delineated as geographical units based on soil and climatic factors. Analysis of 4-year growth data for Phase I Douglas-fir plots indicated that response was not significantly different across provinces. A similar conclusion was drawn from the Phase II province analysis for Douglas-fir. Hazard and Peterson (1984) have documented a statistical model which will be used to test the province response hypothesis using installation averages as the sample unit rather than individual plots as was previously done. Further, the testing will be conducted on the combined Phase I/III and Phase II/III 6-year response databases plus growth data from contract installations. These tests will provide conclusive evidence to help answer the province response questions for cooperators.

Economic Analysis

In addition to conducting mensurational studies, the RFNRP also has performed several economic analyses to determine the financial profitability of nitrogen fertilization in thinned and unthinned Douglas-fir stands. These analyses have generally shown that fertilization of Douglas-fir is an attractive investment when viewed from an incremental perspective. In this form of analysis, the incremental benefits of an activity are compared with the incremental costs of undertaking the activity. Such an analysis is only concerned with measuring the change in economic efficiency as it relates to a single treatment activity. No attention is given to the economic viability of the base investment—only to the incremental costs and benefits. Under a specified set of assumptions, these analyses have produced incremental rates of return in excess of 6 percent (real) on an after-tax basis.

General conclusions drawn from the incremental analysis of merchantable gross CF volume for both thinned and unthinned Douglas-fir stands are: (a) irrespective of fertilization treatment, site classes 2, 3, and 4 should be fertilized in that order; (b) site class 4 is not profitable to fertilize under several of the treatment schedules examined; (c) if only one fertilization is desired, timing nearest to final harvest will be most profitable; (d) if stands are to be held for two investment periods (16 years), then two fertilizations generally are more profitable than one; (e) older stands are more profitable to fertilize than younger stands; (f) 400 lb of nitrogen as urea per acre generally appears to be more profitable than 200 lb, but the optimal dosage level decreases with decreasing site class; and (g) thinned stands generate larger net present values after fertilization than do unthinned stands. Of course, these results only apply if the economic parameters used in the analysis remain valid for any particular user (Bare 1982).

The results of the incremental analysis generally show higher net present values for the more productive sites than for the poorer sites (see conclusion (a)). This follows from the assumption that trees growing on good sites are larger in diameter and have more value per unit volume than trees of the same age growing on less productive land. Thus, even though the volume response to fertilization is not significantly related to site quality, the volume response is more valuable if put on larger diameter trees.

The cooperative has also conducted an absolute net present value analysis of fertilization in thinned and unthinned Douglas-fir. This type of analysis differs from an incremental analysis in that all costs and benefits associated with a proposed treatment are considered in the analysis. Only in this manner can one determine if a stand should be fertilized and held for several years, or terminated immediately. Recall that using an incremental approach the investment is considered to be worthwhile if incremental returns exceed incremental costs. However, this form of analysis is appropriate only if the *base* investment is worthwhile, or if investment (disinvestment) decisions concerning the base investment are

not under control of the manager making the fertilizer decision.

With respect to fertilization, this means that stands with positive incremental net present values should not be fertilized unless: (a) the absolute net present value of the *base* investment is also positive or (b) the forest manager has no control over the timing of the final harvest cut in the stand. If a stand with a positive incremental net present value but a negative absolute net present value is fertilized, it implies a cost-minimization objective and not one of profit maximization.

Results of the absolute analysis of merchantable gross CF volume for both thinned and unthinned Douglas-fir stands clearly show that some stand types with positive incremental net present values (due to fertilization) should not be fertilized, but should be harvested today. Generally, however, if a stand is worth holding for another time period, it is more profitable to fertilize it rather than to leave it untreated. This, of course, assumes that investment funds are available and that they cannot earn higher rates of return elsewhere.

Miscellaneous Results

In addition to the major results discussed above, the cooperative has also conducted a series of special studies focusing on: (a) foliar and stem analysis, (b) effects of fertilization on wood quality, (c) effects of fertilization on specific leaf area and foliage retention, (d) effects of Mount St. Helens ash on the growth of Douglas-fir, (e) correlation of forest soil characteristics with fertilizer response, and (f) mineral cycling in natural and fertilized stands. A full bibliography of these and other RFNRP publications can be found in Bare and Loveless (1985).

The cooperative has also established contract installations on cooperator lands. Approximately 33 of these installations—mostly in southwestern Oregon—have been established to increase sampling intensity on the lands of specific cooperators. A variety of special analyses and reports have been prepared under these contracts.

The large database amassed by the RFNRP is available for use by cooperators who wish to conduct special studies.[2] This valuable source of information has provided the raw material for more than a dozen graduate theses at the University of Washington. It is expected that the value of this database will grow as additional fertilization treatment installations are added.

APPLICATION OF RFNRP FINDINGS

Benefits of the RFNRP include the adoption of the above results by land management agencies and their subsequent impacts on society. Obviously, the principal benefit of the cooperative is the quantification of the effects of nitrogen fertilizer on the growth of thinned and unthinned stands of Douglas-fir and western hemlock. These findings have been adopted by both cooperating companies and agencies as well as organizations which have not directly financed the cooperative.

A recent survey by the RFNRP reveals that 18 forest industry companies and four Federal/State agencies have fertilized nearly 1.6 million acres in western Oregon and Washington over the past 10 years (Chappell and Opalach 1984). All survey respondents indicated that a dosage of 200 lb of nitrogen as urea per acre was applied aerially. For the last half of the decade (i.e., 1979-1983) an average of 155,000 acres per year was fertilized.[3]

Although we have no way of determining how many acres were fertilized annually because of the presence of the RFNRP, it appears that the cooperative has made significant contributions in this regard. This is borne out by the continuing involvement of companies and agencies in the cooperative and their interest in RFNRP research reports detailing new results.

Some companies and agencies have fertilized young stands to increase the harvesting of old-growth reserves and to improve age class distributions. Generally, such organizations operate under some type of sustained yield timber regulation policy which dictates that long-term harvest rates do not exceed growth rates. Thus, with a fixed land base, the only way to increase harvest rates is to increase growth rates. This has led to adoption of the allowable cut effect or earned harvest factor form of analysis by several companies and governmental agencies. Many authors believe that the increased harvesting of old-growth reserves can be directly tied to the fertilization of adjacent young stands and claimed as a direct benefit of the fertilization expenditure. We do not endorse this concept of analysis. However, it is reported herein because this form of analysis has led to the fertilization of young forests

[2] *All requests for data are evaluated by RFNRP staff to determine if the request satisfies the requirements of the cooperative's data-sharing policy.*

[3] *This figure does not include fertilizations occurring on the nonindustrial private ownership land base in the Pacific Northwest.*

in the Pacific Northwest. To the extent that the cooperative contributes to the fertilization decision, it has indirectly had a societal impact.

Closely related to the issue raised above is the use of fertilization to reduce the size of the allowable cut falldown. This phenomenon occurs when a sustained yield timber regulation policy leads to a reduction in old-growth growing stocks over one rotation. Because young stands do not produce volumes per acre equal to those of old-growth stands, there is a sudden reduction in timber harvests as the second rotation begins. One way to reduce this "gap" is to increase the growth rate of the young stands through fertilization. Without passing judgment on the validity of this form of analysis, it is clear that some organizations are fertilizing their young stands with the objective of reducing the size of their allowable cut falldown. Again, to the extent that the cooperative contributes to this decision, it has had an indirect effect on society.

BENEFIT/COST ANALYSIS

One of the more important issues confronting budget analysts and managers of research programs is the benefit/cost performance of research expenditures. These questions arise when new programs are being considered as well as when past or existing programs are being evaluated. In both instances, the critical question is one of measuring the costs of a research program against the benefits expected to flow from the program.

A series of preliminary benefit/cost analyses is developed in this section of the paper. These analyses measure the benefit of fertilization as a joint product of the investment in the cooperative plus the application cost of the fertilizer. A benefit/cost analysis of investments in the cooperative itself is not presented because no reasonable means could be found for allocating project benefits to the investment in the cooperative versus the direct cost of application.

Previously, we indicated that the direct dollar investment in the RFNRP totals $2.5 million as expressed in nominal dollars. To account for inflation, each year's investment is converted into constant (1983) dollars. As shown in table 2, these annual dollar investments have remained relatively stable over the life of the project. For purposes of the benefit/cost analysis, direct dollar investments in the cooperative are continued for an additional 5 years to reflect the anticipated funding for the Phase V extension of the project.

The cost of aerially applying fertilizer averaged $53/acre in 1983 (Chappell and Opalach 1984). This cost is held constant in real terms over time in the benefit/cost analysis beginning with 1983. However, a trend line analysis of past fertilizer application costs clearly shows a decreasing cost from 1973-1983. Thus, fertilizer treatments initiated prior to 1983 are estimated using the results of the trend line analysis (table 3).

The benefit due to fertilization is assumed to be the value of the resultant increase in wood yield. For simplicity, this incremental approach is limited to one fertilization of 200 lb of nitrogen as urea per acre in an unthinned stand of Douglas-fir. The 10-year periodic annual increment as measured in total gross CF volume was earlier shown to be 40 CF/A. Since response is not significantly related to stand age, density or site productivity, the economic analysis applies to an average acre. The 400 CF/A of additional wood is harvested at the end of the 10th year following fertilization.

The stumpage value of the incremental wood yield is held constant at $.50/CF. This is equivalent to approximately $100/MBF using a BF/CF ratio of 4.5/1.0. All benefits are expressed in 1983 dollars and are held constant in real terms over time beginning in 1970. The duration of response from the application of fertilizer is assumed to be 10 years.

It is also assumed that no fertilization decisions are based on results produced by the cooperative until the beginning of the fifth year. Thus, as depicted on the work sheet shown in table 4, the first application of fertilizer does not appear until year 5. Accordingly, the first benefit of $200/acre does not occur until year 14. Similarly, the analysis assumes that a second acre is fertilized at the beginning of year 6, with benefits being realized in year 15. This sequence of application investments and lagged benefits is assumed to continue until the present value of the last benefit falls below $1/acre. Therefore, the length of the investment planning horizon is a function of the discount rate used in the analysis. As previously noted, the investment in the cooperative itself is assumed to terminate in year 20.

All benefit/cost analyses are conducted on a per-acre basis even though more than one acre is involved in the analysis. Hence, the difference in size of the investment introduced by different lengths of investment planning horizons is automatically accounted for. The investment in the cooperative is placed on a per-acre basis by expressing the investment on a per-acre fertilized basis. Because there is no way to determine the number of acres fertilized due to the cooperative's presence, four scenarios are considered. It is assumed that 100, 50, 20, and 10 percent of the 155,000 acres fertilized annually during the past 5 years can be attributed to the cooperative. Thus, per acre investments in the cooperative range from $1.54/acre (in constant 1983 dollars)

Table 2.--Adjustment of RFNRP budget from nominal to constant dollars

GNP Implicit Price Deflators					
Calendar year	1972[1]/ dollars	1983[2]/ dollars	Fiscal year basis	RFNRP budget (nominal dollars)	RFNRP budget 1983 (constant dollars)
1969	86.79	40.158			
			41.236	85,057	206,269
1970	91.45	42.314			
			43.369	100,768	232,350
1971	96.01	44.424			
			45.3415	92,896	204,854
1972	100.00	46.271			
			47.6565	92,543	194,188
1973	105.75	49.042			
			51.2055	96,363	188,189
1974	115.08	53.369			
			55.8525	154,810	277,176
1975	125.79	58.336			
			59.855	155,100	259,126
1976	132.34	61.374			
			63.1615	111,622	176,725
1977	140.05	64.949			
			67.3535	174,507	259,091
1978	150.42	69.758			
			72.7725	195,790	269,045
1979	163.42	75.787			
			79.3165	223,330	281,568
1980	178.64	82.846			
			86.7575	242,013	278,953
1981	195.51	90.669			
			93.3055	194,006	207,926
1982	206.88	95.942			
			97.971	228,664	233,400
1983	215.63	100.00			
			101.0365	307,027	303,877
1984	220.60	102.073			
Total				2,454,495	3,572,737

1/ Source: President's Economic Report, February 1983, and Survey of Current Business, June 1984.
2/ 1983 Dollars = 100.000

Table 3.--Assumed costs of aerial application of 200 lbs. of nitrogen per acre

(1983 Dollars per acre)

Year	Application cost
1974	76
1975	74
1976	71
1977	69
1978	66
1979	63
1980	61
1981	58
1982	55
1983	53
1984-2084	50

1/ Trendline analysis of data from Chappell and Opalach (1984)

Table 4.--Sample worksheet at 12 percent

Fiscal Year	End of year[1]	RFNRP invest/ac[2]	PV RFNRP invest[1]	Application invest/ac[1]	PV application costs	Benefit before tax[1]	PV before-tax benefit[2]	After-Tax Benefit[1] 28% Cap. Gain	Amort.	Total	PV After Tax[2]
70	0	1.331	1.331	0	0						
71	1	1.499	1.195	0	0						
72	2	1.322	0.941	0	0						
73	3	1.253	0.892	0	0						
74	4	1.214	0.772	76.43	48.57						
75	5	1.788		73.79	41.87				7.032	7.032	3.990
76	6	1.672		71.14					13.820	13.820	7.002
77	7	1.140		68.50					20.365	20.365	9.212
78	8	1.672		65.86					26.667	26.667	
79	9	1.736		63.21					32.726	32.726	
80	10	1.817		60.57					31.510	31.510	
81	11	1.800		57.93					30.294	30.294	
82	12	1.341		55.29					29.078	29.078	
83	13	1.506		52.64					27.863	27.863	
84	14	1.999		50.00		200.00	40.924	144.00	26.647	170.647	
85	15	1.539		50.00		200.00	36.538	144.00	25.432	169.432	
86	16	1.539		50.00		200.00	32.624	144.00	23.730	167.730	
87	17	1.539							23.243	167.243	
88	18	1.539	0.200						23.000	167.000	
89	19	1.539	0.179						23.000	167.000	
90	20		Σ =12.400								
91	21										
92	22										
	36				0.846						
	37			50.00	0.755				23.000	167.000	
	38				Σ =357.720				23.000	167.00	
	39							144.00	18.400	162.400	1.950
	40							144.00	13,800	157.800	1.700
	41								9.200	153.200	1.470
	42								4,600	148.600	1.270
	43									144.000	1.100
	44									144.000	.983
	45						1.220			144.000	.878
	46					200.00	1.089			144.000	.784
	47					200.00	.972	144.000		144.000	.70
							Σ =373.8548				Σ =374.46

1/ 1983 dollars
2/ 12 percent

under the 100 percent scenario to $15.40/acre under the 10 percent scenario.

The results of the benefit/cost analysis are summarized in table 5, where both before- and after-tax net present values are shown. The after-tax net present values are based on the assumption of a corporate taxpayer who is subject to a 46 percent tax rate on ordinary income, 28 percent tax rate on long-term capital gains and a 6.5 percent harvest excise tax. As shown in table 6, the cost of fertilizing is amortized over a 5-year period. The results clearly show that under the assumptions employed in the analysis, the incremental investment in fertilization has generated rates of return in excess of 9 percent (expressed in 1983 dollars). The exact internal rate of return earned on the joint investment in the cooperative and the cost of application are displayed in figure 1. As shown, the real after-tax (1983 dollars) internal rates of return are 9.3, 10.7, 11.7, and 12.1 percent for the 10, 20, 50, and 100 percent acreage scenarios, respectively.

The results shown in table 5 agree closely with the results of the economic analyses previously discussed in this paper. This is not too surprising when one remembers that the per acre investment in the cooperative is only about 3 percent of the per acre application cost (assuming the 100 percent acreage scenario) and that the prior economic analyses have shown large positive incremental net present values (excluding the cooperative investment). Thus, from this preliminary assessment, it appears that investments in fertilization in the Pacific Northwest (including both the investment in the cooperative and the cost of application) have produced adequate improvements in the financial posture of the organizations involved. However, our analysis does not reveal whether these organizations are: (a) minimizing losses or maximizing profits through their actions or (b) earning the maximum rate of return on their investment dollars. Answering these questions requires an absolute analysis as discussed in an earlier section of this paper.

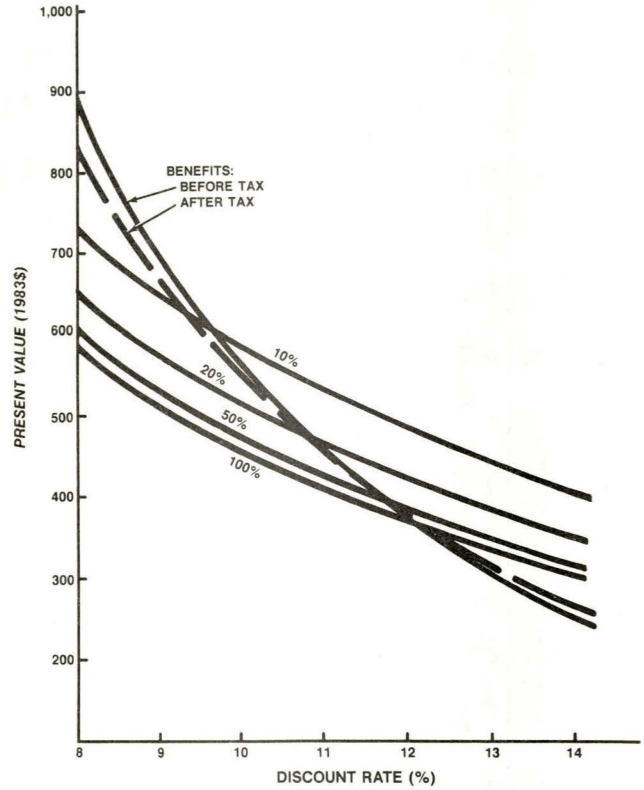

Figure 1.—*Graphical solution showing rate of return.*

Table 5.--Summary of benefit/cost analyses

	INVESTMENTS						BENEFITS		
Discount rate	Cooperative	Application costs	Total[1/] 100%	50%	20%	10%	Before taxes	After taxes[2/]	Years[3/] in planning horizon
-percent--	---dollars---								---years---
6	18.37	788.97	807	826	881	973	1545	1364	90
8	15.92	574.65	591	607	655	734	906	831	68
10	13.97	444.67	459	473	515	584	569	544	55
12	12.40	357.72	370	383	420	482	374	374	47
14	11.12	294.03	305	316	350	405	253	266	40

1/ The columns under total are for different assumptions concerning the amount of acreage fertilized that can be attributed to RFNRP--i.e. 20% assumes that the cost of the RFNRP is spread over 20% of the 155,000 acres fertilized.

2/ A yield tax rate of 6.5%, income tax rates of 46% for corporate ordinary income and 28% long-term capital gains were used. All fertilization expenditures were amortized over 5 years.

3/ Years in planning horizon refers to the length of the horizon used in each analysis. At 6%, the analysis was carried for 90 years, as this is the point at which the present value of the annual before-tax benefit becomes less than $1/A.

Table 6.--Benefits from decreased taxes by amortization[1/]

End of year	Calculation	Fertilization (years amortized)
5	(One-fifth of current year's fertilization cost) x .46 = (1/5) (76.43) x .46 = 15.286 x .46 = 7.632	5
6	15.286 + (1/5) (73.79) x .46 = 30.044 x .46 = 13.820	5, 6
7	30.044 + (1/5) (71.14) x .46 = 44.272 x .46 = 20.365	
8	44.272 + (1/5) (68.50) x .46 = 57.972 x .46 = 26.667	5, 6, 7, 8
9	57.972 + (1/5) (65.86) x .46 = 71.144 x .46 = 32.726	5, 6, 7, 8, 9
10	55.858 + (1/5) (63.21) x .46 = 68.500 x .46 = 31.51	6, 7, 8, 9, 10
11	53.742 + (1/5) (60.57) x .46 = 65.856 x .46 = 30.294	7, 8, 9, 10, 11
	"	"
	"	"
	"	"
	"	"
	etc.	etc.

1/ Sample Worksheet
Amortize the cost of fertilization over a 5-year period beginning the year after fertilization (straight-line, one-fifth of the cost each year, assume a 46 percent Federal ordinary income tax). 1983 dollars per acre.

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AGGREGATE RETURNS TO LUMBER AND WOOD PRODUCTS RESEARCH: AN INDEX NUMBER APPROACH[1]

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Economic evaluations of investments in research have either focused on detailed case studies of specific innovations or else attempted to assess the impact of research on productivity growth at some level of aggregation. Case studies have been useful for building up a stock of knowledge about the effects of major technological innovations and for developing research evaluation methodology. Examples include Griliches' (1958) evaluation of hybrid corn research and the evaluation of industrial innovations by Mansfield *et al.* (1977). But studies such as these are sometimes criticized for considering only highly successful research efforts, thus casting doubt on their representativeness for a given field or industry. To the extent that this criticism is valid, it is unclear what conclusions can be drawn from research evaluation case studies.

Aggregate level research evaluations avoid the "success story" criticism by ignoring individual technologies and instead examining the relationship between research and the growth of productivity in an entire industry or sector of the economy. Broad policy implications are more likely to follow from this type of evaluation. Many aggregate evaluations of production-oriented agricultural research have been carried out, including the early work of Tang (1963), Griliches (1964), and Latimer and Paarlberg (1965).

The purpose of this paper is to describe and apply a simple methodology for evaluating the social returns to research at the level of an entire industry. The index number approach was first developed by Peterson (1971), although the basic idea may be traced to the seminal research evaluation work of Schultz (1953). Briefly stated, the idea is that "research output can be evaluated indirectly by measuring the productivity of the industry toward which the research is directed," (Peterson and Fitzharris 1977). Productivity growth has been used to evaluate agricultural research in several studies. Eddleman (1977) and Lu *et al.* (1978) used projected productivity indexes to estimate future returns to agricultural research. Hastings (1981) examined the relationship between agricultural research and productivity growth, and found a positive relationship between research, climate, and aggregate productivity.

As an illustration of the index number approach, the returns to past research in the U.S. lumber and wood products industry (SIC 24) will be considered. It is found that average rates of return to this research over the period 1942 to 1973 have been quite high. The implications of these findings and several unresolved issues in research evaluation are discussed in a concluding section.

METHODOLOGY AND DATA

Perhaps the most vexing problem in evaluating research from an economic perspective is measuring the value of research outputs. The value of new knowledge—the main output of research—cannot be measured directly. Hence, research evaluators have been forced to use various proxies for research output, including numbers of publications and specific innovations produced by research.

The index number approach uses growth in productivity to indirectly measure research output as the value of resources saved due to more efficient production techniques (Peterson and Fitzharris 1977). For example, suppose that output per unit of input increased by 10 percent between 1970 and 1980 for some industry, and that the value of the industry's production in 1980 was $5 billion. If the inputs used in 1980 had been combined using 1970

[1]*Research supported by the Department of Forest Resources, the Agricultural Experiment Station, University of Minnesota, and the USDA Cooperative State Research Service. The author thanks Professor Hans Gregersen and Professor Willis Peterson for helpful comments and suggestions during the course of this research.*

production technology, $500 million (5 billion times 0.10) less output would have been produced. This $500 million therefore represents the savings in the industry in 1980 due to technical change.

Measuring aggregate research benefits in this way is completely analogous to the approach used in most economic evaluations of research: Technological advances resulting from research cause an upward shift in a production function, which is equivalent to a rise in an index of total factor productivity. Consumer surplus research evaluations use essentially the same approach, except that information about elasticities of supply and demand and the type of supply shift induced by research are included. The beauty of the index number approach lies in its simplicity and directness.

The index of total factor productivity or technical change used to calculate research benefits was derived by Bengston and Strees (1984). This index corrects a bias in a previously reported index of technical change (Robinson 1975) by explicitly including intermediate inputs.[2] It measures increased output of lumber and wood products not accounted for by increases in capital, labor, and intermediate inputs. As shown in figure 1 and table 1, the index was set equal to 1.0 in 1951 and had risen to about 1.4 by the early 1970's. This indicates that due to technical change, it became possible over this period to obtain about 40 percent more output from the same bundle of inputs.

A 3-year moving average was taken to smooth out unusual fluctuations in the productivity index. Wide variations from year to year in an index such as this are likely due to difficulties in measuring the capital input. The swings do not indicate annual changes in productivity. The smoothed index therefore better represents overall trends in productivity growth.

Annual research benefits were calculated by multiplying the increase over the base year of the smoothed index by the annual value of production in the lumber and wood products industry:

$$R_t = [A'_t - 1]\, Q_t \ ,$$

where R_t is gross research benefits in year t, A'_t is the smoothed index of total factor productivity, and Q_t is the value of production in year t (in constant dollars). Research benefits calculated for the years covered by the productivity index are given in table 1.

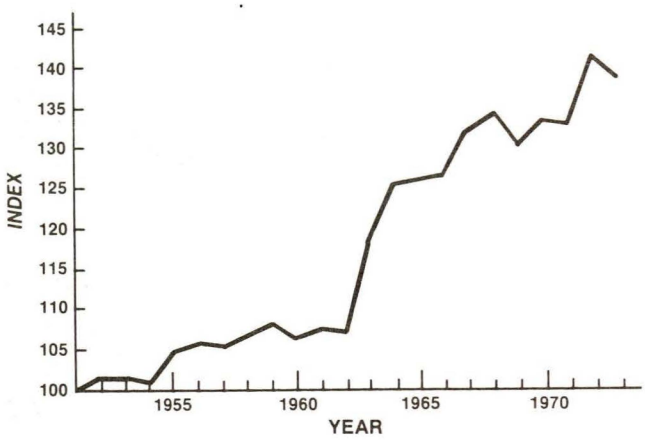

Figure 1.—*Index of total factor productivity, U.S. lumber and wood products industry, 1951-1973; 1951 = 100. Source: Bengston and Strees (1984).*

As previously mentioned, these represent the annual value of real resources saved due to more efficient production technology in this industry.

The derivation of research costs is given in table 2. Annual Forest Service expenditures for forest products utilization research were obtained from Congressional Appropriations Committee documents and Forest Service annual reports.[3] In addition to proposed research expenditures for the current year, actual expenditures for the preceding year are reported in the appropriations hearings. These are shown in the first column of table 2. Research expenditures begin and end 10 years before the calculated stream of benefits, implying a 10-year mean lag between forest products research and an impact on productivity. This assumption was based on the 8- to 10-year lag between structural particleboard research and the resulting economic benefits reported by Haygreen *et al.* (1983).

Utilization research expenditures were expressed in constant dollars using a cost of research index (Davis 1979). Notice that the amount of real scientific resources devoted to Forest Service utilization research actually declined in the years following the massive buildup of this line of research during World War II.

A rough estimate of total U.S. research on lumber and wood products—including the private sector and university research not funded by the Forest Service—was obtained by multiplying the deflated research expenditures by four. This follows the approach of Gregersen *et al.* (1983) in their evaluation

[2]*Intermediate inputs are defined as purchases of goods and services by one firm or industry from another. Examples include raw materials (e.g., wood) and fuel.*

[3]*Forest products utilization research includes research not accounted for in the calculation of benefits, such as pulp and paper. Thus, the estimated expenditures are greater than actual expenditures, and will produce conservative rates of return to lumber and wood products research.*

Table 1.--Estimation of research benefits in the U.S. lumber and wood products industry, 1951-1973

Year	A_t 1/	A'_t 2/	Q_t 3/	R_t 4/
1951	1.0000	1.0000	6,948	0.00
1952	1.0158	1.0106	6.962	73.80
1953	1.0159	1.0119	7,040	83.78
1954	1.0040	1.0234	6,711	157.04
1955	1.0504	1.0373	7,659	285.68
1956	1.0576	1.0540	7,717	416.72
1957	1.0539	1.0604	7,178	433.55
1958	1.0698	1.0695	7,525	522.99
1959	1.0849	1.0732	8,215	601.34
1960	1.0649	1.0751	8,043	604.03
1961	1.0755	1.0707	8,001	565.67
1962	1.0718	1.1092	8,432	920.77
1963	1.1804	1.1684	9,520	1,603.17
1964	1.2530	1.2298	10,010	2,300.30
1965	1.2559	1.2580	10,190	2,629.02
1966	1.2652	1.2810	10,223	2,872.66
1967	1.3220	1.3103	10,779	3,344.72
1968	1.3437	1.3214	11,046	3,550.18
1969	1.2984	1.3241	10,758	3,486.67
1970	1.3302	1.3183	11,030	3,510.85
1971	1.3263	1.3561	11,017	3,923.15
1972	1.4117	1.3735	13,132	4,904.80
1973	1.3826	1.3826	12,702	4,859.79

1/ Index of total factor productivity for the U.S. lumber and wood products industry. Source: Bengston and Strees (1984).
2/ Three year moving average of A_t.
3/ Value of production, U.S. lumber and wood products industry, in millions of 1958 dollars. Source: U.S. Department of Commerce, Bureau of Economic Analysis, unpublished data.
4/ Estimated research benefits in millions of 1958 dollars: $R_t = [A'_t - 1] Q_t$.

of major forest products innovations. Agricultural research evaluators have sometimes multiplied public research expenditures by a factor of two to capture private expenditures (Griliches 1964, Fishelson 1971, Peterson 1971, Peterson and Fitzharris 1977).

Finally, an adjustment was made to recognize that new technologies originating outside a particular industry often have a significant impact on productivity growth within the industry. Previous aggregate research evaluations have not accounted for this potentially major source of productivity growth. Scherer (1982) has examined these interindustry technology flows using data based on patent activity in the U.S. He found that certain industries tend to "borrow" or "import" a significant amount of research results from other industries. The lumber and wood products industry was no exception. Scherer calculated a matrix of interindustry technology flows indicating that for every dollar of research and development (R&D) originating within the industry, 1.8 dollars of R&D is used. Industries such as motor vehicles and equipment, materials handling, machinery, and chemicals accounted for most of the technological innovations flowing into lumber and wood products. The adjustment of total research expenditures for interindustry technology flows is given in the last column of table 2.

Table 2.--Estimation of lumber and wood products research costs, 1942-1963

(Thousand dollars)

Year	Forest Service expenditures[1/]	1958 dollars[2/]	Expenditures[3/]	Adjustment for interindustry technology tech. flows[4/]
1942	966	2,322	9,288	16,718
1943	1,075	2,506	10,024	18,043
1944	1,064	2,254	9,016	16,229
1945	1,144	2,460	9,840	17,712
1946	1,402	2,850	11,400	20,520
1947	1,536	2,893	11,572	20,830
1948	1,243	2,114	8,456	15,221
1949	1,179	1,946	7,784	14,011
1950	1,187	1,815	7,260	13,068
1951	1,266	1,798	7,192	12,946
1952	1,261	1,675	6,700	12,060
1953	1,246	1,556	6,224	11,203
1954	1,207	1,418	5,672	10,210
1955	1,256	1,394	5,576	10,037
1956	1,565	1,623	6,492	11,686
1957	1,927	1,875	7,500	13,500
1958	2,402	2,402	9,608	17,294
1959	2,618	2,395	9,580	17,244
1960	2,854	2,501	10,004	18,007
1961	3,527	2,954	11,816	21,269
1962	4,477	3,562	14,248	25,646
1963	4,822	3,678	14,712	26,482

1/ Forest products utilization research expenditures in current dollars. Sources: (1) Department of the Interior and related agencies appropriations for (various years), hearings before a subcommittee of the Committee on Appropriations, House of Representatives. (2) Department of Agriculture appropriations for (various years), hearings before the subcommittee of the Committee on Appropriations, House of Representatives. (3) Report of the Chief of the Forest Service, Statistical Supplement (various years).

2/ Forest Service forest products utilization research expenditures deflated by a research price index (Davis 1979, p. 138).

3/ Estimated as four times Forest Service expenditures. Includes estimated expenditures by firms, universities, and other public agencies.

4/ Estimated as 1.8 times total expenditures (see text).

RESULTS

Internal rates of return were calculated for the investment in U.S. lumber and wood products research over the period 1942 to 1973. It was found that this line of research has been highly profitable from society's point of view. A return of 40 percent was calculated when estimated total research costs (not adjusted for interindustry technology flows) were compared to research benefits. When the estimated cost of developing technologies used in forest products but originating in other industries is included, the rate of return is 34 percent.

DISCUSSION

A frequent theme in the agricultural research evaluation literature is that the high rates of return compared with most other types of investments indicates an underinvestment in agricultural research. There is a fallacy in this argument, however, as pointed out recently by Fox (1984). Research evaluators have calculated social returns and compared these with the private returns obtainable by firms. But social and private returns are incommensurate. In fact, Peterson (1976) has shown that the social rate of return to private research is necessarily higher than the private return. Conclusions about a general underinvestment in forest products research therefore cannot be drawn from this study.

Furthermore, the average rates of return calculated in this study preclude the use of the results in research resource allocation. Average rates of return to research investments are helpful in better understanding the economic impacts of R&D. In research decisionmaking, however, marginal decisions may be made at any juncture. A marginal rate of return—providing information about the return to additional investment—is therefore the appropriate economic measure for comparing the merits of alternative research projects or programs.

What general conclusions can then be drawn from the relatively high rates of return to lumber and wood products research? First, investment in this type of research has been socially profitable. The gains to society stemming from this research have been substantial compared with costs.

Second, returns to lumber and wood products research compare favorably with returns to aggregate agricultural research. Peterson (1971) calculated an average rate of return to U.S. agricultural research over the period 1910 to 1967 of 19 percent. To be strictly comparable, similar time periods for the two investments should be considered. Although not directly comparable, estimated marginal rates of return to aggregate agricultural research have generally been found to be in the range of 35 to 40 percent (Griliches 1964). Some aggregate-level evaluations have found higher marginal rates of return for certain periods (Evenson *et al.* 1979, Davis and Peterson 1981), while others have found lower returns (Latimer and Paarlberg 1965, Fishelson 1971).

The aggregate rates of return calculated in this study are also of the same order of magnitude as the average returns found in previous forest products research evaluations. Gregersen *et al.* (1983) found rates of return ranging from 18 to 30 percent when the benefits of several major forest products innovations were compared with estimated total forest products research expenditures. Bengston (1984) found average returns to structural particleboard research in the U.S. of 19 to 22 percent.

Several issues have been raised in this paper which require further attention. Perhaps most interesting is the question of accounting for the costs of interindustry technology flows in evaluating research at an aggregate level. This is less likely to be a concern in a case study evaluation of a specific innovation, but at the aggregate level it is clear that innovations originating in one industry may be a major source of productivity growth in another. A current example is the proliferation of specialized microcomputer technology and its impacts on the productivity of timber processing, farm management, etc. Attributing all productivity gains to research carried out within an industry may result in a substantial upward bias in the returns to research. Evaluations of agricultural research have not adjusted research expenditures for interindustry flows, even though industries such as pharmaceuticals (other than agricultural chemicals), rubber and plastic products, machinery (other than farm machinery), and motor vehicles and equipment have been important sources of innovations used in agriculture (Scherer 1982).

A related issue is the impact of international technology flows on productivity. Very little work has been carried out in this specific area, although a large body of literature exists on international technology transfer (Sagafi-nejad and Belfield 1980). In most cases, technological imports would not affect rates of return to research. The accounting stance of most research evaluations is a particular country, so research costs incurred outside of the country are not relevant. The costs of bringing the technology into the country should be included, however, and Mansfield (1975) has shown that international technology transfer is by no means costless. In addition, Mansfield *et al.* (1982) have pointed out that U.S. firms carry out a significant amount of research abroad that has an effect of productivity growth in the U.S. If these research expenditures are borne by the U.S. parent firm, they should also be included in an evaluation.

Another unresolved issue in the evaluation of forest products research is the appropriate length and shape of the time lag for the impact of research expenditure on output. This is an issue usually discussed in production function research evaluations, but information about the research lag must be included in other types of evaluations as well. Davis (1980) has reviewed alternative research lag structures used in agriculture. Empirical work is needed

in forest products to determine the length and distribution of the research lag. Research on the diffusion of innovations in forest products (Globerman 1976, Stier 1983) may be useful in this endeavor.

Finally, an important question in aggregate research evaluation relates to the inclusion of private research expenditures. Peterson (1976) has argued that since firms capture a return to their R&D by charging higher prices for their output, there is a "...double counting of the cost of private R&D when assessing the social rate of return to this investment". The cost would be included "...once on the cash outflow side and would appear again on the return side as a reduction in cash inflow (social returns)". This issue obviously has important implications for the way in which research costs should be treated, and deserves more attention. Assuming that private research expenditures in lumber and wood products are half the estimated total, the rate of return to this research increases to 47 percent when private costs are omitted. Note that it would be inappropriate to adjust research expenditures for interindustry technology flows if Peterson's argument is valid. If private research costs within an industry should not be included on the expenditure side to avoid double counting, then private costs incurred in other industries should also be excluded—that is, firms in technology "exporting" industries capture a return to their research investments by charging higher prices to the "importing" industries.

CONCLUDING COMMENTS

This paper has illustrated the use of a relatively simple approach to evaluating aggregate research investments. The index number approach described here is quite "user friendly"—the methodology is easily grasped by evaluators and by the intended users of evaluation results, unlike many other research evaluation methods. In addition, data requirements are light. This may be a particularly important factor in attempts to develop evaluation methods for forestry and forest products research, where the availability of data is often a critical problem.

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DIFFUSION OF INNOVATIONS IN FORESTRY AND FOREST PRODUCTS: REVIEW OF THE LITERATURE[1]

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The impacts of new technologies on society depend on the extent to which the technologies are utilized and the pattern of their adoption over time. Information about the diffusion of innovations is therefore fundamental to the evaluation of research. A large amount of work has been done in this area: The processes by which innovations are diffused throughout social systems have been studied extensively by scholars in many fields. Thousands of empirical and theoretical investigations into the diffusion process have been carried out over the past several decades (Rogers *et al.* 1977).

Most research evaluations have used simple assumptions to project the spread of innovations over time, and in some cases the calculated rates of return to research investments are highly sensitive to these assumptions. Although sophisticated diffusion models do not guarantee more reliable results, it is clear that forestry research evaluators could benefit from taking a closer look at what is known about the diffusion of innovations.

For example, the diffusion of containerized forest tree seedlings has proceeded much more slowly in the United States than in Canada and Scandinavia (Westgate 1984). About 50 percent of all planting stock is containerized in Canada and Scandinavia, but containerized seedlings account for only 6 percent of U.S. production. Understanding the factors that have created this large difference in adoption levels and thereby speeding up the diffusion process would have a significant impact on the returns to containerized seedling research.

This paper reviews and assesses the diffusion of innovations literature with a special focus on the forest-based sector. It is organized into three sections. First, to make sense of this broad-ranging and multidisciplinary area of study, a framework for interpreting diffusion research is presented: Diffusion research is characterized by several descriptive continua and three major traditions of diffusion research are identified and briefly described. The second section consists of a review of forestry and forest products diffusion research, with studies grouped according to their respective research tradition. A concluding section discusses the implications of this review and needs for future work in this area.

One caveat is in order before proceeding. Many studies have been carried out that examine the characteristics of forest landowners in relation to their use of various forestry management practices. These studies are in some ways similar to diffusion of innovation studies, but they remain outside the scope of diffusion research because they do not employ the distinctive conceptual models developed by researchers in this area. To keep the focus on the diffusion literature per se, these studies will not be considered in this review.

A FRAMEWORK FOR DIFFUSION RESEARCH

Many different types of diffusion research can be identified. Figure 1 characterizes diffusion research according to three continua, which cover the most prominent and relevant features of diffusion research. First is the individual/aggregate adoption continuum. Research on individual adoption is concerned with the processes by which an individual adopting unit (a person, firm, or organization) decides to adopt or reject an innovation. The focus is on the individual decisionmaking process and the relevant characteristics of the adopting or rejecting units. Researchers in fields such as sociology, communications, anthropology, and psychology have tended to concentrate on individual level adoption.

At the other end of the spectrum, "Aggregate adoption is measured by the aggregate level of use of

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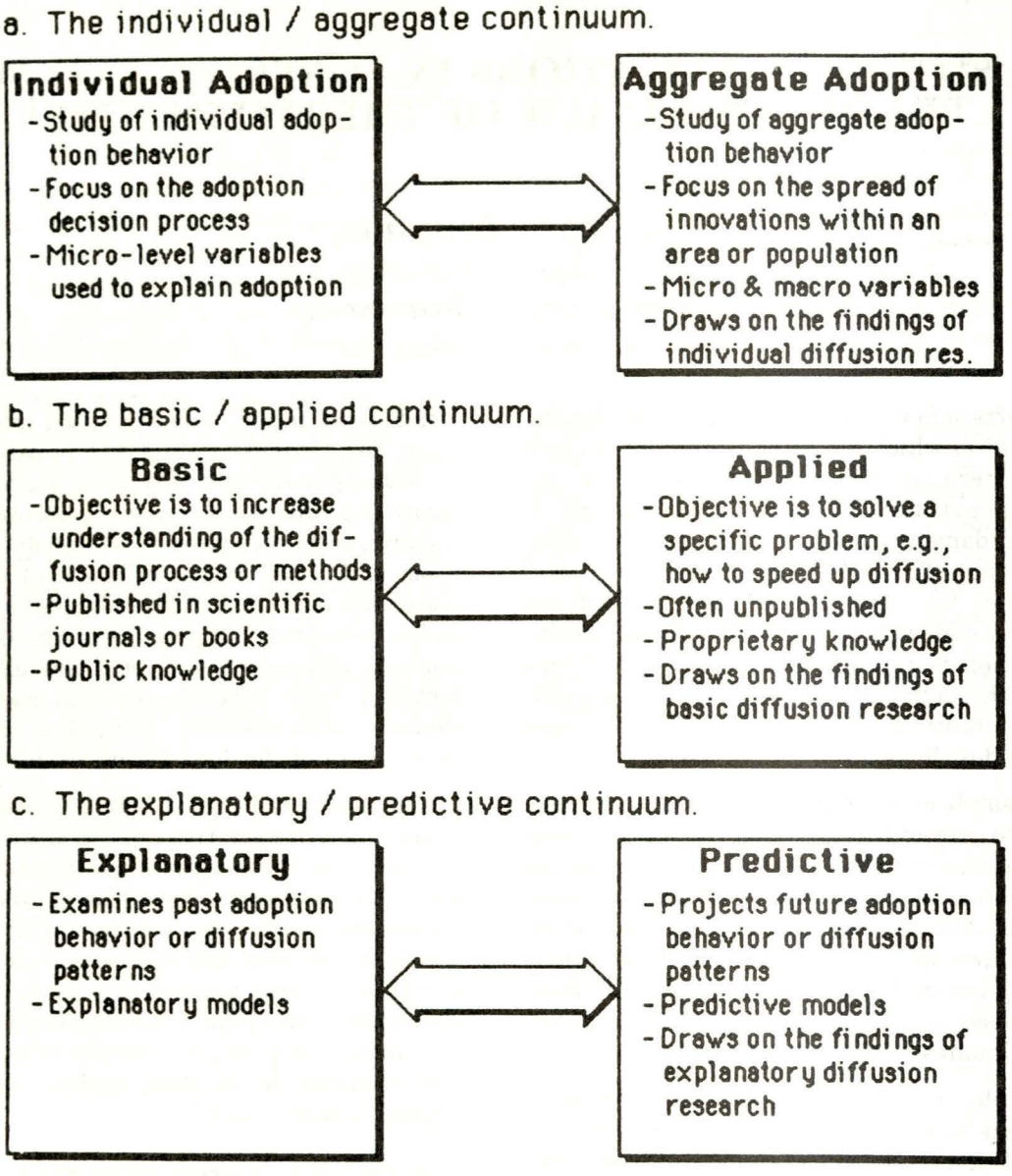
a. The individual / aggregate continuum.
Individual Adoption
- Study of individual adoption behavior
- Focus on the adoption decision process
- Micro-level variables used to explain adoption
Aggregate Adoption
- Study of aggregate adoption behavior
- Focus on the spread of innovations within an area or population
- Micro & macro variables
- Draws on the findings of individual diffusion res.
b. The basic / applied continuum.
Basic
- Objective is to increase understanding of the diffusion process or methods
- Published in scientific journals or books
- Public knowledge
Applied
- Objective is to solve a specific problem, e.g., how to speed up diffusion
- Often unpublished
- Proprietary knowledge
- Draws on the findings of basic diffusion research
c. The explanatory / predictive continuum.
Explanatory
- Examines past adoption behavior or diffusion patterns
- Explanatory models
Predictive
- Projects future adoption behavior or diffusion patterns
- Predictive models
- Draws on the findings of explanatory diffusion research

Figure 1.—*Three diffusion research continua.*

a specific new technology within a given geographical area or within a given population" (Feder *et al.* 1982). Studies of this type often try to explain the pattern of adoption of an innovation over time. Aggregate adoption research builds on the microfoundation provided by individual adoption research, but in addition to characteristics of the adopting unit, characteristics of the innovation and the environment in which diffusion takes place are often considered as explanatory variables. Economists and geographers have typically concentrated on aggregate level adoption.

A second way of categorizing diffusion research is along the basic/applied continuum, as shown in figure 1b. Basic diffusion research is aimed at increasing understanding about the processes underlying diffusion. Theoretical models of diffusion which are not tested empirically would be included at this end of the continuum.

The theories of basic diffusion research are tested empirically by applied researchers. Applied diffusion research has the objective of answering specific questions regarding case studies. For example, an analyst might be interested in estimating the eventual market share that will be captured by an innovation, measuring the rate of adoption, or identifying important factors in the diffusion of a given innovation.

Finally, diffusion research may also be characterized as falling along an explanatory/predictive continuum. Explanatory models examine past adoption

behavior or diffusion patterns. Independent variables are chosen which provide the best explanation of the observed behavior. A classic example is Mansfield's (1961) study of industrial innovations.

Predictive diffusion models, on the other hand, attempt to forecast certain diffusion variables, such as the rate of diffusion or the level of adoption. Independent variables are chosen to be useful for predictive purposes, i.e., they must be observable during the early stages of diffusion. See Martino *et al.* (1978) for an example.

As a means of further organizing the diffusion literature, Kelly and Kranzberg (1978) have suggested that diffusion research may be divided into three overall traditions on the basis of similarity in conceptual structure and methodology: sociology-psychology (S/P), economics, and geography. Several characteristics of each tradition are summarized in figure 2. The S/P tradition of diffusion research includes a number of academic disciplines, including anthropology, communication, psychology, and sociology. Inclusion of these disciplines makes this the oldest and by far the largest body of diffusion research.

Several common threads bind the S/P tradition together. First, the diffusion of innovations is viewed as a process that can be explained primarily by social, cultural, and psychological variables. The attitudes and sociocultural characteristics of adopters are often stressed as the most important factors in determining adoption behavior. Thus, work in the S/P tradition has a strong tendency toward the individual adoption end of the individual/aggregate continuum. Another common trait of this tradition is general agreement on the importance of social networks in the diffusion process. A classic example is the 1957 study by Coleman *et al.*, which explored the influence of social networks among physicians on their adoption of a new drug.

Finally, a common conceptual underpinning of the S/P tradition is provided by the "classical" diffusion model. As formulated by Rogers (1983), the classical model represents a synthesis of the approach to diffusion research that originated in the early 1940's with the work of rural sociologists Ryan and Gross (1943). In its quintessence, the classical model consists of four basic elements thought to be central to the diffusion process: "(1) an innovation, (2) communicated via certain channels, (3) to members of a social system, and (4) who adopt it over a period of time" (Rogers and Eveland 1978). A detailed treatment of each element is given in chapters 2 through 11 of Rogers (1983).

Economists have also brought their perspective to bear on the diffusion of innovations. The work in this area has been much more limited and recent than the S/P tradition, with the first economic study appearing in 1957 (Griliches 1957). Four main fields of

Research Tradition / Characteristics	Sociology/ Psychology	Economics	Geography
Main Disciplines	Anthropology Communication Psychology Sociology	General Economics Agricultural Econ. Marketing Tech. Forecasting	General Geography Economic Geography
Individual/ Aggegate Adoption	Focus on individual adoption.	Focus on aggregate adoption.	Focus on aggregate adoption.
Explanatory /Predictive	Focus on explanatory models.	Explanatory and predictive models.	Focus on explanatory models.
Common Variables	Social, cultural, and psychological variables.	Economic variables.	Spatial and economic variables.
Major Empirical Studies	Ryan & Gross (1943) Coleman et al. (1957)	Griliches (1957) Mansfield (1961)	Hagerstrand (1952)

Figure 2.—*Summary of diffusion research traditions.*

study are included in the broadly defined economics tradition of diffusion research: general economics, agricultural economics, technological forecasting, and marketing.

Several generalizations about the economics tradition may be ventured. First, the diffusion of innovations is viewed as a process that can be explained mainly by economic variables. Economic variables considered most frequently include profitability and size of the initial investment required to adopt an innovation. These are two aspects of relative advantage, a much broader explanatory variable that has been examined by diffusion researchers in each of the three traditions.

Economists have also tended to focus on aggregate adoption. This concentration on the aggregate level is in contrast to the S/P tradition in which the decisionmaking process involved in individual adoption is often stressed. Different research approaches and models have been used to focus on aggregate adoption. Instead of personal interviews and detailed sociometric data, economists have usually relied on secondary data and econometric models to test their hypotheses concerning diffusion.

The third major stream of diffusion research is the geographer's tradition of spatial diffusion—that is, "The relationship between the innovativeness of adopters and their relative position in physical space" (Kelly and Kranzberg 1978). Geographers have studied the diffusion process through most of this century (Brown 1981). The early work in this area was carried out by cultural geographers whose main concern was the role of the spread of innovations in shaping the cultural landscape. Geographic diffusion research took a different tack following the pathbreaking work of Swedish geographer Torsten Hagerstrand in the early 1950's. A new group of geographers—trained mainly in economic and urban geography—were inspired by Hagerstrand to investigate the diffusion process.

Several generalizations about the geographic tradition may be made. First, the physical proximity of potential adopters is stressed as a variable to explain observed diffusion patterns. Researchers in the geography tradition have also investigated other variables, termed receptivity factors, which are thought to modify the spatial pattern and rate of adoption of innovations. These include the adopter's attitudes and values, cost of adoption, returns to adoption, and the role of self-interested innovation propagators (Hagerstrand 1968).

Like many researchers in the economics tradition, geographers have tended to focus on adoption at the aggregate level. According to Hagerstrand (1968), the geographers have been concerned with various "macro-concepts" such as "growth curves, centers of innovation and centers of spread, channels of diffusion, barriers to diffusion, cultural boundaries, and regional differences in receptivity."

The standard conceptualization of spatial diffusion—developed by Hagerstrand—is based on the idea that the adoption of an innovation is the outcome of a learning or communication process (Brown 1981). This is congruent with the basic tenets of the S/P tradition. As a means of testing this conceptualization, Hagerstrand introduced Monte Carlo simulation techniques to model the diffusion process over time and space (Hagerstrand 1967). This innovation in diffusion research has been adopted by many researchers in the geography tradition.

DIFFUSION RESEARCH IN FORESTRY AND FOREST PRODUCTS

In this section diffusion research in the forest-based sector will be reviewed and characterized according to the framework presented in the preceding section. Empirical and non-empirical studies of the diffusion of forestry and forest products innovations will be reviewed as comprehensively as possible. Sociology/psychology, economic, and geographic studies will be grouped separately to facilitate evaluation.

The Sociology-Psychology Tradition

Several studies have examined the diffusion of forestry innovations from the S/P perspective. Most of these have been carried out by rural sociologists. Perhaps the first forestry diffusion study of this type was an analysis of the adoption of improved forestry practices by nonindustrial private forest land (NIPF) owners in Louisiana (Bertrand and South 1963). The classical diffusion model provided the conceptual underpinning for this study. Bertrand and South were primarily interested in determining the characteristics of small woodland owners related to their propensity to adopt recommended forestry practices. Diverse forestry practices were chosen as the innovations to be studied, including pruning trees, fencing out livestock, constructing fire guards, preparing ground for natural seeding, using a written contract for selling wood, and seeking advice from professional foresters. The results of the analysis were summarized as follows:

> Those characteristics found to be related positively to adoption of woodland management practices were: a progres-

sive attitudinal orientation; a median age (40-59); white race; male sex; the attainment of higher education (up through high school); larger-sized holdings both total and in woodlands; the perception of benefits derived from woodlands; upper socioeconomic rank; a mixed farming enterprise; full-time farming; and residence in Washington Parish.

It was concluded that the results of this study would be useful to foresters interested in promoting new practices. Unfortunately, the usefulness of this analysis is limited by the fact that the relative importance of the above factors on the decision to adopt was not measured.

In addition, as is often the case in diffusion studies of this type, most of the factors considered by Bertrand and South have little or no relevance for public policy. Examples include the race and socioeconomic status of adopters. Public policymakers could not use these variables in the design and implementation of a technology transfer program.

Technology transfer as a field of study is closely related to diffusion research. The findings of diffusion research are often applied in technology transfer studies with the explicit goals of (1) speeding up the rate at which an innovation is adopted, (2) increasing the extent of adoption, or (3) otherwise increasing the efficiency and effectiveness of the diffusion process. Examples of technology transfer work in forestry include Moeller and Heytze (1981), Nicholls and Prey (1982), and Hobbs *et al.* (1983). Because the technology transfer literature is large and fairly distinct from the diffusion literature, it will not be considered further in this paper.

A spate of similar studies followed Bertrand and South, including Sollie (1965), South *et al.* (1965), Sollie (1967), and South (1968). Each of these studies routinely tested a series of hypotheses about the characteristics of adopters of recommended forestry practices in limited geographic areas. The conclusions reached were generally quite close to those of Bertrand and South (1963).

In addition to these empirical studies, Muth and Hendee (1980) provided an overview of the S/P tradition of diffusion research from a forestry perspective. They concluded that the findings of diffusion research could contribute significantly to the design of technology transfer programs to speed up the adoption of forestry innovations.

The Economics Tradition

Several investigations into the diffusion of forest products innovations have been carried out by economists in the past decade. These studies have looked at three types of innovations: pulp and paper, wood-based panels, and residential construction. The earliest and most comprehensive economic study was Hakanson's (1974) analysis of the diffusion of special presses used in paper making. Using an econometric model similar to Mansfield's (1963), Hakanson studied the diffusion of four different types of special presses in six countries. He hypothesized that the profitability of adoption, date of first information about special presses, firm size, and an innovativeness index would be positively related to early adoption by a firm. Hakanson found that the profitability and company size variables explained a large amount of the variability in adoption rates.

Globerman (1976) studied the diffusion of special presses in the Canadian paper industry. Two econometric models were developed to determine which factors were most important in explaining the diffusion of special presses. First, a model of inter-firm diffusion was specified and fit. It was found that higher profitability of special press adoption was positively related to early adoption, firm size was not related to time of adoption, and domestically owned firms were slower to adopt than foreign subsidiaries. Globerman also fit a model of intra-firm diffusion designed to identify factors associated with the spread of innovations within a firm after initial adoption. In this model domestic or foreign ownership was found to be unimportant in explaining the spread of special presses within a firm. Another economic study of the diffusion of innovations in the Canadian paper industry—including special presses—was carried out by Martin *et al.* (1979b).

Two paper-making innovations were included in a large diffusion study of 49 innovations in 14 industries by Martino *et al.* (1978). The kraft pulping process and computer process controls in the pulp and paper industry were examined, although the former innovation was not included in the final analysis due to insufficient data on some industry variables. The intent of this study was to develop a predictive model to forecast the rate of diffusion across industries. Conclusions specifically related to the diffusion of computer process controls in paper making cannot be drawn from this study because the data for each of the included innovations were pooled to estimate the predictive models. But the *ex ante* approach and large number of independent variables examined makes this an instructive and interesting study, particularly for researchers concerned with forecasting diffusion.

Finally, Stier (1983) examined the diffusion of the sulfate pulping process in the U.S. pulp and paper industry. Stier developed an econometric diffusion model based on the Gompertz function. An innovative feature of this model was the inclusion of a dynamic diffusion ceiling, in which the ceiling level of adoption was assumed to be a log-linear function of real product price and real gross national product. It was concluded that "most of the rapid expansion in sulfate pulp production that occurred can be attributed to upward shifts in the ceiling level of demand," and not in the "natural rate of diffusion." Projections made for the years 1990 and 2000 show that "the sulfate process will continue to capture market share and could account for over 90 percent of total U.S. woodpulp production by the year 2000."

Several economists have studied the diffusion of innovations in wood-based panels. An interesting example of international diffusion is provided by the particleboard case study by Buongiorno and Oliveira (1977). They analyzed the growth of the share of particleboard production (measured as a percentage of total wood-based panels) in 25 industrialized countries using an approach similar to that of Griliches (1957). Buongiorno and Oliveira concluded that countries in which particleboard was initially adopted slowly tended to have higher long-term ceiling levels. It was also found that eastern European countries had significantly lower ceiling levels of adoption than market economies. An econometric analysis of factors influencing the date of initial adoption in a country concluded that greater distance from the Democratic Republic of Germany (where particleboard was first produced) and the availability of large supplies of roundwood were the most important factors in delaying the time of adoption of particleboard. Finally, it was found that countries with higher economic growth and lower wood availability tended to have more rapid rates of adoption.

Leefers (1981) examined the diffusion of particleboard and southern pine plywood in the U.S. He was mainly interested in the effects of the diffusion of these panels on wood requirements in the future. Two diffusion models—the logistic and Gompertz functions—were used to estimate the potential market shares and growth rates for panel innovations. He concluded that these univariate models, while useful for depicting diffusion trends over time, were not helpful for explaining the diffusion process. Multivariate models were therefore used to identify supply and demand factors influencing particleboard and southern pine plywood diffusion.

Residential construction has been identified as a neglected sector in the study of technological change (Strassmann 1978). There have been several investigations into the diffusion of innovations in residential construction, however, including wood products innovations. Martin *et al.* (1979a) examined the diffusion of wood roof trusses in Canada. They concluded that the diffusion of roof trusses has been closely related to cycles in residential construction. This is hardly surprising, but it reinforces Stier's (1983) findings that increases in overall demand can induce much more rapid diffusion. It was also concluded that institutional constraints—such as zoning laws—are not impediments to the diffusion of roof trusses, and that proximity to large local markets is an important factor due to high transportation costs.

In a study of innovation in residential construction in the U.S. (Spall 1971), roof trusses, finger-jointed wood, and six other innovations were studied. Spall's analysis was based on interviews of the managers of 20 firms building single-family housing in Michigan. He concluded that the entrepreneur's extent of adoption was not correlated with his years of formal education, his experience in the industry, his subjective receptiveness to innovations, the size of his firm and volume of business, or the possibility of increased competition. Adoption or nonadoption was found to depend only on estimates of profitability of adoption. These findings contradict the conclusions of diffusion researchers in the S/P tradition.

Finally, a total of 14 innovations in residential construction—including, once again, wood roof trusses, several other wood-related innovations, and a variety of nonwood innovations—were examined by Ventre (1979, 1980). He considered an innovation to be "adopted" in an area when local building codes were modified to permit its use. The main finding of this study was a refutation of the common perception of the housebuilding industry as technologically backward:

> The major significant result of this analysis is to deny the technological lethargy of the building industry and the agencies that regulate it: innovations diffuse throughout the industry—despite the special characteristics cited at the beginning of this paper—in much the same way and at the same speed as in other industries (Ventre 1979).

The Geography Tradition

Judging by reviews of several large diffusion bibliographies and a bibliography of geographic diffusion studies (Harton *et al.* 1975), it appears that very little research into the diffusion of forestry and

forest product innovations has been carried out in the geography tradition. In fact, we have been unable to locate any forestry diffusion studies which employ the distinct methodological approach to diffusion research developed by geographers. There are, however, several studies by geographers which examine the spatial patterns of technical change in forestry.

Dinsdale (1965) attempted to define the changing spatial patterns in the lumber industry of northern New York State and relate these to three distinct phases of technological development. The three phases were first identified by Mumford (1934), and are summarized by Dinsdale as follows:

> The eotechnic or "dawn age" of modern technics is typified as a wood, wind, and water power complex; the paleotechnic is the coal, steam, and iron complex; and the neotechnic is briefly characterized by such features as electricity, the internal combustion engine, alloys and synthetic materials.

Dinsdale traced each of these technological phases in the lumber industry and identified the technological innovations associated with each. She concluded that each phase of the industry's development was characterized by spatial and technological patterns which conform well to Mumford's phases of the industrial revolution.

Adeyoju (1969) analyzed the development of the timber industry in Benin with a focus on the spatial and temporal spread of timber concessions, an institutional innovation. This is essentially an historical account, however, rather than a geographic diffusion case study. It was concluded that physiography, rainfall and technological developments in transportation systems were important factors in the early growth of the industry.

CONCLUSIONS AND IMPLICATIONS

Several conclusions emerge from this review of forestry and forest products diffusion research, which is summarized in figure 3. First, relatively little work has been done on forestry innovations per se. Work in the S/P and geography traditions on forestry innovations appears to be very limited. All of the research by economists has looked at innovations in forest products, probably because these innovations are quite amenable to economic analysis. But many forestry innovations relate to nonmarket activities.

Empirical Studies	Type of Innovation	Diffusion Research Tradition
Bertrand & South (1963) Sollie (1965) South et al. (1965) Sollie (1967) South (1968)	Improved Forestry Practices	Sociology/ Psychology
Hakanson (1974) Globerman (1976) Martino et al. (1978) Martin et al. (1979b) Stier (1983)	Pulp & Paper	Economics
Buongiorno & Oliveira (1977) Leefers (1981)	Wood-Based Panels	Economics
Spall (1971) Martin et al. (1979a) Ventre (1979, 1980)	Residential Construction	Economics

Figure 3.—*Summary of forestry and forest products diffusion research.*

It is clear that more research is needed to understand the diffusion of forestry—as opposed to forest products—innovations. One of the few studies contributing to this area identified 22 factors that were important in the successful adoption of forestry innovations (Moeller and Shafer 1981). The most important factors were found to be adaptation of existing technology, pilot studies to demonstrate feasibility, cooperation with private industry, and the development of underlying theory.

Another conclusion from this review of the literature is that predictive diffusion models are lacking. Forecasting the diffusion of forestry and forest products innovations would be useful to research evaluators, policy analysts, and policymakers. The only existing predictive models of diffusion developed specifically for the forest sector are Stier's (1983) model of technological substitution in the pulp and paper industry and Leefer's (1981) models for woodbased panels.

It would appear that the diffusion of innovations literature has several important limitations for application in forestry at the present time. The number of forestry-specific diffusion studies is small and the extent of the coverage is limited with respect to type of innovation studied, geographic area, etc. The potential usefulness in terms of increasing the rate of adoption and implementing technology transfer policies is restricted by the variability of research findings, the high degree of specificity, and the paucity of policy-relevant variables in existing empirical studies. Finally, usefulness of diffusion research for forecasting in forestry is limited by a shortage of predictive models.

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SUMMARY: EVALUATING RETURNS TO RESEARCH IN TIMBER-HARVESTING TECHNOLOGY

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Research in the commercial production of forest products includes a broad spectrum of subjects. Topics range from tree seedlings and regeneration to final consumer products such as housing and furniture. Timber harvesting and transportation provides an important link between the biological production of standing trees and their manufacture into primary and secondary forest products.

Project MIS-6901 of the Mississippi Agricultural and Forestry Experiment Station is designed to evaluate returns to research in timber-harvesting technology. The study of aggregate investments in timber-harvesting research will be evaluated for two major timber-producing regions, the South and the Pacific Northwest. Harvesting technologies vary significantly between these regions due to many forest-type and physiographic differences.

Most timber-harvesting research and development in the U.S. has resulted in labor-saving technical change. For this reason the initial framework for evaluation will involve *ex post* production function methods. These methods have been applied extensively in agricultural research evaluations, and involve predicting aggregate output with conventional inputs and research expenditures. Logging industry (SIC 2411) measures in the Census of Manufactures include value added and value of shipments, value of depreciable assets, new capital expenditures, and various measures of labor as possible variables for output and conventional capital-labor inputs. Secondary data are not available, however, for aggregated harvesting research. One option for the analysis involves developing and using approximate measures of regional research efforts in harvesting. Another option is to exclude research inputs and concentrate on defining productivity increases, using indirect measures to impute returns to research and development.

Production function methods can also be used to consider the distribution of research benefits over time. Several agricultural studies, for example, have assumed research benefits increase, reach a maximum, and decrease with time, and have modeled the relationship with a second-degree polynomial. The Office of Technology Assessment recently reported that 7-10 years may be required to develop new, innovative systems in timber harvesting. In the present study, problems may arise in modeling lagged research effects, since cross-sectional data are anticipated. Agricultural evaluations with similar problems will be considered in evaluating production function approaches and their application to the research lag for timber-harvesting improvements.

Preliminary results of the study should be available by the spring of 1986. The analysis will then be refined, and equity and distributional issues will be investigated.

AN ECONOMIC ANALYSIS OF A NEW GROWTH AND YIELD MODEL FOR OAKS IN NEW ENGLAND

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Oak-hickory stands cover 108,900,000 acres—some 22.6 percent of the commercial timberland in the United States. Geographically, oak-hickory stands extend in a widening band from southern New England to the grasslands of Nebraska, Kansas, Oklahoma, and Texas. Oak-hickory stands in New England cover 2,146,000 acres of commercial timberland (USDA Forest Service 1982).

Until recently, however, neither a local yield table nor a local growth and yield model of this commercially important forest type had been developed for the New England region. Land managers in New England have historically relied on Midwest guidelines (Gingrich 1971) when making management decisions. These guidelines do not accurately reflect New England conditions, and, as Hibbs and Bentley (1984) have indicated, the volume and value of oak-hickory forest in New England has increased to the point where small errors in management prescriptions can have a large economic impact. In this paper we will investigate the value of a recently published growth and yield model for oaks in New England (Hibbs and Bentley 1984).

In determining the economic value of the local growth and yield information, we considered only the direct value. Indirect effects, such as possible increases in employment, income, and the multiplier effects thereafter, were not included. Further, since the growth and yield information only addresses the question of timber volume, we limited our analysis to impacts on timber production.

MEASURING THE VALUE OF LOCAL GROWTH AND YIELD INFORMATION

Forest mensurationists and biologists would argue that gathering and developing growth and yield information is an endeavor to produce scientific knowledge. The obtained knowledge itself has its intrinsic value. From an economic point of view, however, the answer may not be so clear. Whether a growth and yield model has any value at all depends on how it affects timber management decisions. If the new information leads to better management decisions, there must be economic gains, and the new growth and yield information thus has economic value. On the other hand, if the new information does not change management decisions, there is no improvement, no economic gain, and thus no value accrued.

Just how should the economic value of the growth and yield information be measured? Since the objective of timber production is to maximize the land expectation value, it would be natural to measure the value of the local growth and yield information through changes in the land expectation value as a result of their availability. For example, if the optimal regional rotation age is 35 years when the stumpage price is $58/MBF, regeneration cost is $90/acre, and the interest rate is 6 percent, then the hypothetical local yield table (table 1) would lead to a rotation age of 33 years under the same combination of price, regeneration cost, and interest rate. The value of the local growth model, in terms of increased land expectation value would then be $10.63. This $10.63 is derived as follows: When the stand is harvested at age 35, the stand volume would be 27.5 MBF rather than the yield reported in the regional yield table. With a yield of 27.5 MBF, the value of the land would be $120.01. Once the hypothetical local yield table becomes available, the landowner would shift his rotation to 33. The land expectation value, as a result, becomes $130.40 Since the local yield data would result in a better management decision and the landowner would have gained $10.63 on a per acre basis it could be said that the $10.63 represents the value of the local yield information per acre. On the other hand, if the regional rotation were 33 then the hypothetical yield table has no economic value at all. It does not have economic value because management decisions are the same with or without it.

Table 1.--The hypothetical local yield table and the land expectation value calculation[1/]

Year	Production	Land expectation value
	boardfeet/acre	$/acre
25	10,700	62.40
26	12,000	71.22
27	13,400	79.55
28	15,100	90.00
29	17,100	101.98
30	19,200	112.71
31	21,400	122.51
32	23,500	128.69
33	25,300	130.64
34	26,600	127.14
35	27,500	120.01

1/ Assumptions for calculation of LEV:
Stumpage price, P(t), = $58 per thousand boardfeet
Regeneration cost, c, = $90 per thousand boardfeet
Interest rate, r, = 6%

THE VALUE OF THE LOCAL GROWTH AND YIELD MODEL FOR OAK STANDS IN NEW ENGLAND

Typically, oak stand management is divided into two phases (Holsoe 1933). In phase I, during which the most rapid growth in total tree heights occurs, high stand density is maintained to promote natural pruning of branches and consequently rapid increase in merchantable height. In phase II, stand density is reduced periodically by thinning to allow unconstrained crown growth and accelerated stem diameter growth.

We would argue then that if the new growth and yield model led to choices for phase I length, phase II length, and after-thinning growing stock levels different from those indicated by the Midwest guidelines, such a model has indeed had economic value. On the other hand, if all management decisions remain the same with or without the new growth and yield model, then the new model basically has no economic value.

Over the years, the Midwest guideline represented the *de facto* regional yield table. All management decisions were based on the yield information contained therein. As the first step in measuring the value of a local yield model for oak in New England, the optimal management regime under the Midwest guideline must be determined. To facilitate later comparison with the results from Hibbs' and Bentley's study, the same price, precommercial thinning cost, and interest rate were used to determine the length of phase I period and final harvest age. Cordwood price was assumed to increase linearly from $5 to $10 per cord as d.b.h. increased from 5 to 10 inches, and to remain constant at $10 per cord for trees above 10 inches d.b.h. Saw log stumpage price was assumed to increase linearly from $120/MBF to $300/MBF as d.b.h. increased from 12 to 30 inches, and then remain constant. The precommercial thinning cost was assumed to be $50 per thinning (a thinning is considered precommercial when the stand diameter is less than 5 inches). Interest rate was assumed to be 4.5 percent.

Based on the yield table in Gingrich's (1971) study, the optimal combination of phase I length and final harvest age for a site index 65 oak stand is 30 and 80 years, respectively. When this combination is applied to the growth and yield model for upland oak stands with the same site index in New England, the land expectation value is only $52.16 per acre (Hibbs and Bentley 1984). The optimal combination of phase I duration and final harvest age for New England, on the other hand, is 45 and 95 years, respectively. The land expectation value in this case equals $100.81 per acre. Therefore, it can be said that the value of the new growth and yield model equals $48.65. However, it must be pointed out that this $48.65 represents the value of the new model when the stumpage price, interest rate, and precommercial thinning cost are as described earlier. What happens to the value of the growth and yield information when these parameters change? The most likely

changes are summarized below (it is possible that these statements will not hold true in all cases):

1. The higher the stumpage price, the higher the value of growth and yield information.
2. The higher the precommercial thinning cost, the lower the value of the growth and yield information.
3. The higher the interest rate, the lower the value of the growth and yield information.

MEASURING THE BENEFIT/COST RATIO OF GROWTH AND YIELD STUDIES

When we examine the benefit/cost ratio of a particular growth and yield study, additional considerations enter into both the benefit and cost sides of the computation. On the benefit side, there is first the question of the level of adoption—the higher the level of adoption, the higher the total benefit. Second, there is the question of timing of the adoption; obviously, the earlier the adoption the greater the benefit. On the cost side, we have to consider not only the cost of the study itself, but also the cost of disseminating the results. If the ultimate users are not aware of the research results, then there will never be any adoption.

In the case of the oak growth and yield study for New England, we made the following rough estimates. First, based on conversations with experienced foresters, we assumed a site index of 65 is average for all oak sites in New England. Second, we assumed that landowners would adopt the new management regime on only one-fourth of the oak sites. We further assumed that the rate of adoption is such that the overall benefit will be further reduced by three-fourths. Based on these rather conservative estimates, the total benefit of this oak growth and yield study equals

$$\$48.65 \times 2{,}146{,}000 \times 0.25 \times 0.25 = \$6{,}525{,}181.20.$$

While we do not have the actual cost of the project, it has to be less than $400,000 (roughly 3 years' worth of the entire McIntire-Stennis fund for Connecticut). A benefit/cost ratio of 16.31 certainly would be highly acceptable to any research administrator.

CONCLUSION

In this study, we examined the impact of the newly published oak growth and yield model for New England. Highly favorable results were obtained because:

1. There was no real regional yield table for oaks in New England, and they grow quite differently from oaks in the Midwest.
2. There is a very substantial acreage of oak in New England.
3. The stumpage price for oak is high.

Based on these observations, it would seem prudent in the future to follow a research strategy that favors growth and yield studies of:

1. Those species without a true regional model.
2. Those species with a large acreage.
3. Those species with high stumpage value.
4. Those species with low regeneration and other silvicultural costs.

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RESEARCH EVALUATION TECHNIQUES APPLIED TO A CASE STUDY OF SHORT-ROTATION FORESTRY

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The forestry community has planned and conducted forestry research for well over 100 years, and continues to provide new information and technologies for managing and using forest resources. Yet, we have only a shallow understanding of the decision making processes within forestry research and the organizational structure of researchers within the broader forestry community. We also lack systematic methods for evaluating the effectiveness and efficiency of forestry research.

As part of the larger U.S. Forest Service program in research evaluation, we recently studied the research process within one area of endeavor—short-rotation forestry. This case study was undertaken by Fege and Brown (1984) to document the resources invested and the results obtained from 1966 to 1982, to identify factors that influenced the development of this area of research, and to evaluate efforts to implement research results.

Three aspects of that case study are described in this paper: the relationships between funds and publications on an annual basis, as measures of research inputs and outputs; the relationships between total funds and publications; and the level of participation of investigators from one 5-year period to the next, as measured by funds and publications. In addition, the availability of data is addressed as well as the applicability to forestry research of various research evaluation methods used in the case study. Other aspects of the case study reported elsewhere include a survey of individual researchers to identify their reasons for conducting research in short-rotation forestry, the use of publications to describe the organizational structure of this research community, and an assessment of short-rotation technology development (Fege and Brown 1984).

The research effort in short-rotation forestry exhibits several attributes that are characteristic of forestry research in general, making this area of research a good choice for a case study. These attributes include: (1) an interdisciplinary approach; (2) an urgency attached to the initial research efforts and a relatively rapid infusion of funds, similar to that in the Forest Service's spruce budworm or southern pine beetle programs; (3) the involvement of several funding and performing agencies outside of the Forest Service and forestry schools, similar to that in acid rain research; (4) the ability to apply a body of basic information and known research methods in addressing the research questions; and (5) the involvement of both "well-established" and "new" researchers. Short-rotation forestry also lends itself well to a case study on the research process because the concept can be defined fairly narrowly, all research has been conducted since the concept was defined in 1965, most investigators are still active in forestry research or administration, and no specialized training exists or is necessary for "entry" into short-rotation forestry research from other disciplines.

There have been many definitions of short-rotation forestry and many phrases used to describe the same concept: short-rotation hardwoods, silage sycamore, minirotation forestry, intensive culture, silvicultural biomass farms, and energy plantations. For this study, we used the following commonly accepted definition: genetically improved planting stock established at spacing of 2×2 m or less, managed by intensive cultural techniques to optimize total biomasss production, harvested and regenerated at least every 10 years, and used for energy, pulp, or reconstituted wood products.

AVAILABILITY AND RELIABILITY OF DATA

The delineation of research directly or indirectly relating to short-rotation forestry was difficult, even though we thought that we had selected a fairly narrow, well-defined area of forestry research. Many of the problems encountered in collecting data for any research evaluation are related to the definition of research area. Whether the data are needed for the evaluation of a narrow field, such as short-rotation

forestry research, or a larger field, such as silviculture or forest management, it is always difficult to decide which information is peripheral and therefore should be excluded.

We used investigator groups as our unit of analysis for the aspects of the case study described in this paper. Research is commonly undertaken by groups of several scientists, technicians, and graduate students. Publications are commonly authored by one or more of these individuals plus graduate students and other contributing researchers. We found data on publications relatively easy to obtain. The initial list of publications was expanded by asking for additions and corrections from individual investigators, and further expanded with a list compiled by Oak Ridge National Laboratory scientists (Jack Ranney, personal communication). After selecting the publications, we analyzed literature citation patterns and coauthorship patterns in the case study (Fege and Brown 1984).

It is always difficult to identify the source and amount of funds allocated to a specific research area. The most reliable data on sources of funding were obtained from researchers, but the discrepancies in accounting procedures among universities and agencies limited the accuracy of funding level information. Researchers often report only operating costs, whereas the total funds allocated include indirect costs and salaries. As a practical matter, most scientists are conducting several lines of research at any given time, and may commit time and resources to studies that are slightly peripheral to the research for which they are receiving funds. In return, the scientist may write the final publications for a line of research when he or she is receiving funds for an entirely new area of research. Also, it is particularly difficult to identify research efforts within interdisciplinary fields such as watershed management or silvicultural control of various forest pests. Large research projects, such as the larger multifunctional research work units in the Forest Service, may be working on several problems at once and cannot isolate the funds dedicated to any one species or silvicultural method. Furthermore, research that can be applied to several species or stand types or pest groups cannot be separated, and errors in funding estimates are generated when someone arbitrarily assigns the funds to individual species or pests.

PERFORMANCE OF INVESTIGATOR GROUPS

Since a research group generally has a longer life than any one of the individual members, we chose to evaluate the publications of entire groups of investigators as part of the case study. Within our detailed study of 436 articles on short-rotation forestry published between 1966 and 1982 (Fege and Brown 1984), key words were used to identify the research group responsible for each publication. The pattern of published research results rarely parallels the pattern of funding received by a group of researchers. As table 1 shows, one investigator group published short-rotation articles for 4 years before receiving funds designated for this research; two groups were funded by as many as four agencies in 1 year; other groups (not illustrated in table 1) received funds from only one agency during their research in short-rotation forestry. In general, groups at land grant universities received funds from the greatest number of sources.

We assessed the contributions of 21 investigator (each indicated by a letter) groups by several different evaluation techniques. Table 2 shows the rank of each group on one measure of input (year of first funding) and therefore the same rank within a column (groups joined by a vertical line are listed alphabetically). To simplify interpretation, dotted lines mark the groups that had fairly similar rankings within the higher rank numbers. Table 3 shows the relative rank of each group on three measures of input (year of first funding, total number of years of funding, and total funds received).

Groups identified as B, C, and R consistently had low rank numbers, indicating a greater number of publications or more funds or earlier entry into the field of short-rotation forestry. An entire block of groups—G, I, J, L, N, and Q—was consistently ranked higher than the average group (within the dotted lines in tables 2 and 3). Groups A, D, and E received the lowest total funding, but groups A and E were among the groups receiving funds for the greatest number of years. All three groups, however, were among those with the greatest number of publications and earliest date of first publication. Groups F, O, and T had the smallest number of publications (four or fewer total publications in short-rotation forestry), yet groups F and O were among the groups with the highest total funding. Groups H and S were among the first to receive funds and received funds for more years that the average group, but these two groups were average with respect to the year of first publication and number of publications. Group K had the second largest number of publications, but otherwise was within the "average" range marked by the dotted lines. Group M wrote more publications and received more funds than the average group, but entered short-rotation forestry research later and received funds for fewer years.

Table 1.--Funding levels[1/] and publications[2/] for selected investigator groups, by year and funding agency

Investigator	Year													
Code[3/]	67-68	69	70	71	72	73	74	75	76	77	78	79	80	81
B	1		1					1	4	1	3	4	1	
		a	a	a	a						a	a	a	a
											ee		eee	eee
		s	s	s	s						s	s	s	s
													oo	oo
C	1		2	1	1	4		3	12	6	12	9	2	6
									eee	eee	eee	eee	eee	eee
				ff	ff	ff	fff	fff	fff	fff	fff	fff	fff	fff
D					3	2	2	1	2	2		8	4	7
									aa	aa	aa	aa	aa	aa
											ee	ee	ee	ee
H											3	2	2	2
			a	aa	aa	aa	aa	aa	aa	aa	aa	aa	aa	aa
											ee		ee	ee
									o	oo	oo	oo	oo	
N												1	3	3
										a	aa	aa	aa	aa
											ee	ee	ee	ee
													o	o
R	1			2	2	2	3		3	1	3	1	5	4
										eee	eee	eee	eee	eee
	ss	ss	ss	ss	ss	ss	ss	ss	ss	ss	ss	ss	ss	ss
									1			1	4	
						a	a	a	a	aa	aa	aa	aa	a
											ee			
					i	i	i	i	i	i	ii	ii	i	i
					s	ss	ss	ss	s	ss	ss	ss	ss	ss

1/ Funding levels indicated by letters. Codes for funding agencies and levels of funding.

a = Agricultural experiment station
e = U.S. Department of Energy
f = U.S. Forest Service
i = Industry
o = Other agencies
s = State funds
single letter=$500 to 10,000/year
two letters=$10,001 to 100,000/year
three letters=$100,000 or more/year

2/ Number of publications indicated by number

3/ Letters represent separate groups of investigators (not identified in article). Same codes used for investigator groups in other tables and figures.

Table 2.--Ranking of short rotation forestry investigator groups, by year of first funding, year of first publication, and total number of publications

Rank	Year of first funding[1/]	Year of first publication[2/]	Total number of publications[3/]	Year of first funding
1	R[4/]	P	C	R
2	P	R	K	P
3	B	B	R	B
4	H	C	D	H
5	C	A	B	C
6	S	D	M	S
7	A	K	A	A
8	E	E	E	E
9	L	S	U	L
10	J	F	N	J
11	N	G	H	N
12	F	H	I	F
13	G	I	J	G
14	I	M	S	I
15	K	N	P	K
16	M	O	Q	M
17	O	Q	L	O
18	Q	J	G	Q
19	T	L	O	T
20	U	U	F	U
21	D	T	T	D

1/ Groups with lower rank numbers received their first funds for short rotation forestry research earlier than groups with higher rank numbers.

2/ Groups with lower rank numbers published their first article on short rotation forestry earlier than groups with higher rank numbers.

3/ Groups with lower rank numbers published more articles on short rotation forestry than groups with higher rank numbers.

4/ Letters represent separate groups of investigators (not identified in the article). Same codes used for investigater groups in other tables and figures. Lines drawn between the letters representing each group show relationships between the measures. Dotted lines mark groups that had fairly similar rankings with the higher rank numbers. Vertical lines indicate those groups with the same values (such as year of first funding) and therefore the same rank within a column.

STRUCTURE OF THE RESEARCH COMMUNITY

Our efforts to represent the structure of short-rotation forestry research graphically were inspired by some of Derek de Solla Price's work (personal communication) and by techniques developed to evaluate science citations (Garfield 1979). De Solla Price refined a concept of an "invisible college" or cluster of researchers: most communication between researchers occurs within a group of about 100 people. He suggested that the structure of research areas could be represented by relationships among researchers, such as citation maps and maps of coauthorship patterns. In discussing the specific structure of the short-rotation forestry community, one colleague familiar with this community (David Fege, personal communication) suggested to us that some researchers had always been in the "in" group, and some had always been on "the fringe." We translated this observation into "participation maps," to

Table 3.--Ranking of short rotation forestry investigator groups, by year of first funding, total number of years with funding, and total funds received

Rank	Year of first funding[1/]	Total number of years with funding[2/]	Total funds Received[3/]	Year of first funding
1	R[3/]	R	C	R
2	P	H	R	P
3	B	C	F	B
4	H	B	M	H
5	C	E	B	C
6	S	S	H	S
7	A	L	J	A
8	E	J	N	E
9	L	A	O	L
10	J	N	U	J
11	N	F	P	N
12	F	G	G	F
13	G	I	S	G
14	I	K	T	I
15	K	M	I	K
16	M	O	K	M
17	O	P	L	O
18	Q	T	Q	Q
19	T	U	D	T
20	U	Q	A	U
21	D	D	E	D

1/ Groups with lower rank numbers received their first funds for short-rotation forestry research earlier than groups with higher rank numbers.

2/ Groups with lower rank numbers received funds for short rotation forestry research during more years than groups with higher rank numbers.

3/ Groups with lower rank numbers received more funds for short rotation forestry research during the years 1967 to 1982 than groups with higher rank numbers.

4/ Letters represent separate groups of investigators (not identified in the article). Same codes used for investigater groups in other tables and figures. Lines drawn between the letters representing each group show relationships between the measures. Dotted lines mark groups that had fairly similar rankings with the higher rank numbers. Vertical lines indicate those groups with the same values (such as year of first funding) and therefore the same rank within a column.

express the participation of investigator groups in funding and publication over time (figs. 1 and 2).

We characterized the level of participation of each investigator group by the annual funding level and the number of publications within a 5-year period. We named these various categories "IN," "OUT," and "FRINGE," and assigned each group's level of participation to one of these categories. Investigators with the most funds and publications were considered "IN" and those without funds or publications were considered "OUT." We assigned the "FRINGE" category to those investigators receiving very small amounts of money for short-rotation forestry, or publishing only a few articles within a 5-year period. Twenty-one investigator groups are represented in these figures by letters; these letter designations were also used in tables 1 and 3. Group C was one fund-receiving entity but included four distinct subgroups of researchers who published frequently; these were separately designated C1 through C4.

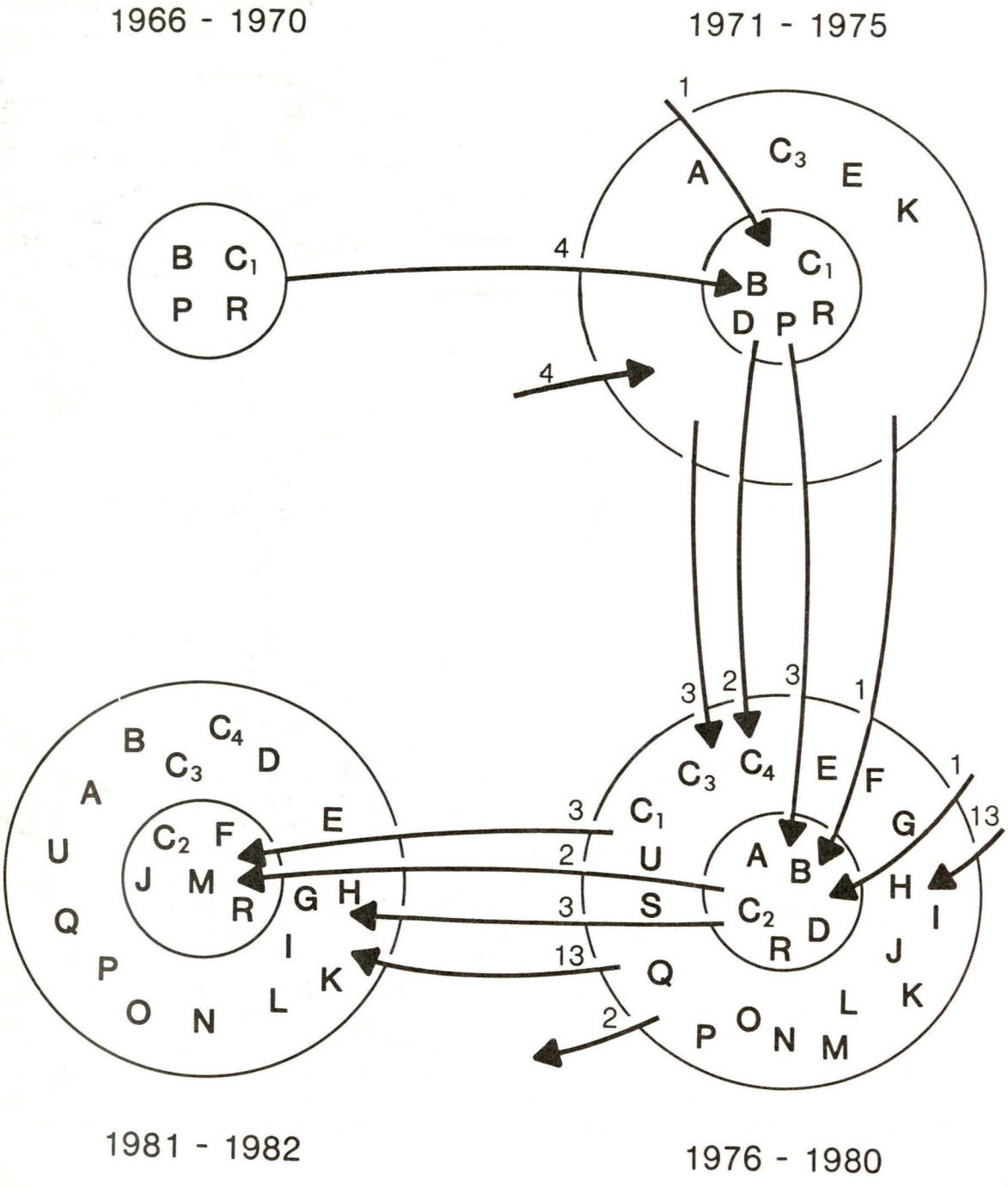

Figure 1.—*Time-participation map for publications of 21 researchers in short-rotation forestry.*

Legend: Letters within in inner circles represent the "IN" researchers or research groups in short-rotation forestry. All researchers publishing in 1966 to 1970 were considered "IN". For the years, 1971-1975 and 1980-1981, "IN" researchers published 5 or more articles, and for the years 1976-1979 published 10 or more articles. Outer circles represent the "FRINGE" researchers, who published less than the 5 or 10 articles. Investigator groups not represented within the inner or outer circles for any time period did not publish any short-rotation forestry articles. Arrows show the number of groups moving between the levels of publications, from one time period to the next.

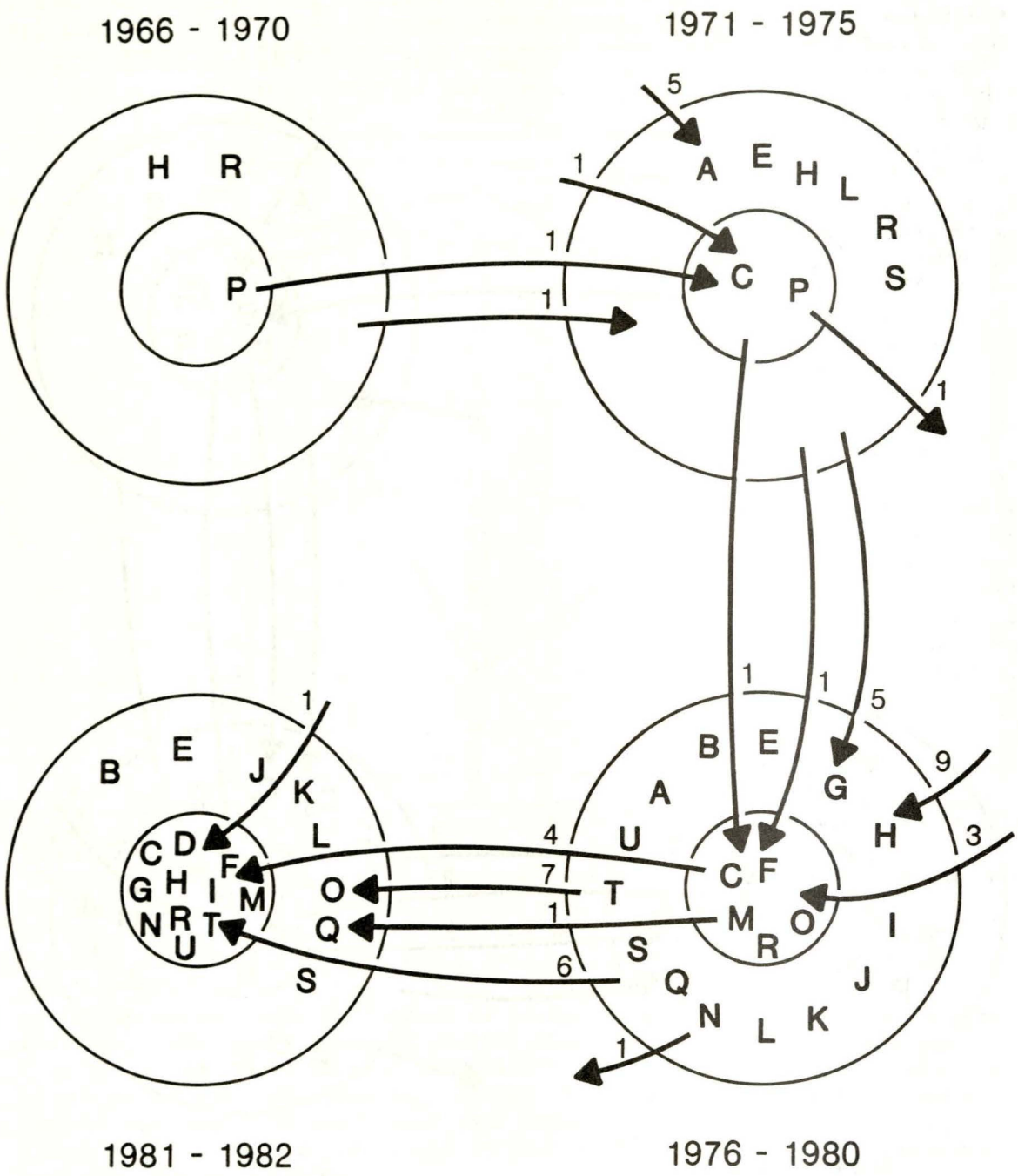

Figure 2.—*Time-participation map for funds received by 21 researchers in short-rotation forestry.*

Legend: Letters within the inner circles represent the "IN" researchers or research groups—those receiving more than $100,000 annually (in years that they received some funds). Outer circles represent the "FRINGE" researchers—those receiving less than $100,000 annually for short-rotation forestry research. Investigator groups not represented within the inner or outer circles for any time period did not receive any funds for short-rotation forestry research during that time period. Arrows show the number of groups moving between the various funding levels, from one time period to the next.

We have drawn several observations about the structure of short-rotation forestry research from these time/participation maps. Figures 1 and 2 suggest that only about half of the "IN" groups are the same for any 5-year period; that is, the groups receiving the most funds did not always publish the most articles. As shown in figure 2, there has been a fairly constant flow of groups into the short-rotation forestry research community, with the greatest increase between the second and third time periods (between 1971-1975 and 1976-1980), coincident with the increased availability of funds. Most of the "recruitment" of groups into the "IN" category was from the "FRINGE" category, and most groups entered the "FRINGE" category from the outside. There are some exceptions: groups A and D entered the "IN" category (for publications) directly from the outside, and groups C, F, M, and O entered the "IN" category (for funds) directly from the outside. Since the cutoff point for being in the "IN" category in terms of funds was $100,000, part of the increase in the number of "IN" groups for funding for the 1981-1982 period can be attributed to five groups that received slightly less than $100,000 annually in 1976-1980 and slightly more than $100,000 in 1981-1982.

The longevity of some researchers and investigator groups in short-rotation forestry extends nearly 20 years, and many individuals have now had nearly 10 years of experience. However, some of the most influential (or most frequently cited) publications were written by scientists who wrote relatively few publications on short-rotation forestry (Fege and Brown 1984). Although most of these scientists probably did not shape the future of short-rotation forestry, their experiences expanded the knowledge of short-rotation forestry concepts within forestry and other research communities.

LESSONS ON RESEARCH EVALUATION METHODS

We consider the application of a range of evaluation measures to the same area of research to be one of the strengths of the entire case study, only part of which is summarized in this paper. By using several different evaluation methods, we were able to confirm many of our observations. For example, conclusions about the organization of short-rotation forestry research were derived from intensive interviews with four groups of researchers, from funding histories, and from analyses of coauthorship patterns. Insights gained from interviews with individual scientists were confirmed by our documentation of the funding and publication patterns.

We consider our familiarity with short-rotation forestry research, our participation in the structure of this research community, and our personal acquaintance with individual researchers to be largely positive aspects of this study. The overall time and effort required to complete the study was much less than it would have been if we had not been familiar with short-rotation forestry, and there was little "start-up" time at the beginning of the study. We tried to keep personal biases and previous experiences with individual scientists and research projects out of the study, although we recognize that perfect objectivity is not possible.

With a view toward applying evaluation methods tested and developed in this case study to other areas of forestry research, we tried several common research evaluation methods developed in other fields of science. These included documenting funds, personal interviewing, surveying active researchers, characterizing publications, and analyzing citation patterns. To our knowledge, few of these have been applied to forestry research, and then only one or two at a time. We are unaware of any other attempts to describe the organization of the research community by coauthorship patterns, to compare several measures of the "productivity" of research groups at the same time, or to examine the participation of research groups within the community over several periods. Describing other areas of forestry research by coauthorship patterns and participation maps should enhance our understanding of the structure of forestry research, and give us further information about how our findings in short-rotation forestry research can be generalized to other areas.

Administrators are commonly interested in evaluating programs from data that are easily and rapidly collected. This has led them to rely almost exclusively on publications. Indeed, the emphasis on publications as the principal measure of "output" became one of the limitations of our study. We made no effort to assess the "usefulness" of individual publications, by either peer judgment or other criteria. With respect to dissemination of research information, we looked only at publications, not at the actual application of the information by users. We had expected that feasibility studies would be a measure of "output," but found that we could not quantify the structure of those studies or the accuracy of production cost data to our satisfaction. There is a need to develop other measures of research productivity and accomplishments that are based on readily available data, as well as to develop methods for using publications to gain more accurate information about research programs.

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VARIABILITY IN RESEARCHER PRODUCTIVITY

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LITERATURE REVIEW

Studies on researcher productivity have shown that there are great differences among individuals in the rate of scientific production. One early study still being cited is Lotka's (1926) analysis of the number of scientific papers produced by physicists. He developed the "inverse square law of scientific productivity," which states:

$$F(n) = \frac{K}{n^2},$$

where n is the number of papers, F(n) is the number of scientists producing n papers, and K is a constant. The distribution for this function is highly skewed, so that for every 100 scientists producing one publication, there are 25 scientists producing two publications, 11 scientists producing three publications, and so on. This leads to the case where three-quarters of the scientists produce only one-quarter of all publications while one-tenth of the scientists produce approximately one-half of all publications (Leikola 1981).

Although later studies on researcher productivity analyzed additional measures of productivity, publications remain one of the more tangible and measurable products of the research process. Pelz and Andrews (1966) collected information on the individual histories and performance of more than 1,300 scientists and engineers in 11 research and development laboratories. They also analyzed laboratory conditions at each location. Performance was measured subjectively (judgments by colleagues in the same laboratory) and objectively (a 5-year count of number of papers in professional journals, patents and patent applications, and unpublished manuscripts). Pelz and Andrews analyzed the relationship between performance and 12 factors: freedom, communication, diversity, dedication, motivation, satisfaction, similarity to colleagues, creativity, age, climate, coordination, and groups. For each relationship the authors offered suggestions as to how research administrators could manage their organizations to enhance productivity.

Data relating the quality of the research published by a scientist to the quantity of research published were presented by Cole and Cole (1968). From the population of university physicists, they drew two samples. First, they sampled 120 physicists, with an over-representation of eminent scientists, to determine (1) number of papers published (used as a measure of publication quantity), (2) number of awards received, (3) number of citations (used as a measure of publication quality), and (4) nationally assessed rank of their department. They then sampled 2,036 physicists to determine the extent to which the work of the scientists in the first sample was known, and the prestige of the awards received by the scientists in the first sample. For physicists, they found a strong correlation between the quantity and quality of the research published. However, they went on to determine that citation counts provided a better measure of quality than number of publications alone.

Cole and Cole proposed two factors to explain the relationship between quantity and quality of research published. First, it is necessary to engage in a large number of research efforts to produce quality research. Second, the scientific reward system operates so that scientists producing quality research are encouraged to continue to produce, while scientists producing research of limited significance are diverted to other areas. When a scientist's work is used by his or her colleagues, that scientist is encouraged to continue doing research; when work is not used, the scientist's efforts decline.

In 1971, Whitley and Frost (1971) summarized and evaluated the progress made in measuring research performance. They approached the problem from a sociological perspective and contended that "...Current problems of measuring performance are not primarily those of devising more sophisticated methodologies, but of arriving at a coherent theoretical framework indicating what is to be measured." After examining various measures of researcher productivity, they concluded: "Although it may be true

that over the entire population of publishing scientists the number of papers produced in the individual's lifetime is related, though not very highly, to the evaluation of his peers...at any one time the mere number of published papers by a researcher is not a valid indicator of the quality of his work."

Cole and Cole (1972) discussed factors affecting scientific productivity when they attacked the Ortega hypothesis. The Ortega hypothesis asserts that without the work of average scientists who make only minor contributions to science, the true breakthroughs by outstanding scientists would be impossible. Cole and Cole find fault in the two basic assumptions of the hypothesis: (1) that the ideas of the average scientist are visible to and used by outstanding scientists, and (2) this minor work is necessary for major breakthroughs. In their analysis, they found that 50 percent of all scientific papers are produced by 10 percent of the scientists. While 15 to 20 percent of the work cited in papers of major breakthroughs is produced by average scientists, Cole and Cole argue that the prevalence of independent multiple discoveries indicates that all scientists are replaceable—although some have more "functional equivalents" than others (Cole and Cole 1972). They suggest that it "...may not be necessary to have 80 percent of the scientific community occupied in producing 15 or 20 percent of the work that is used in significant scientific discoveries, if perhaps only half their number could produce the same work."

Allison and Stewart (1974) picked up on earlier work by Merton (1968) and Cole and Cole (1972) to study accumulative advantage. They stated that because of a variety of social mechanisms, productive scientists are likely to be more productive in the future, while scientists who produce little original work are liable to decline further. The data they collected from chemists, physicists, and mathematicians supported the idea of accumulative advantage. They found that fewer than 6 percent of the publishing scientists typically produced 50 percent of all scientific papers. Allison and Stewart stated that accumulative advantage works for two reasons. First, scientists who are recognized for scientific achievement are motivated by their colleagues' expectations to maintain or increase their recognition. Also, with recognition comes increasing access to resources, which enhances scientific achievement.

In a 1976 study of the Forest Service Research Branch (known as the CRUSK study), Lingwood and Morris (1976) defined productivity as contributions to the field or discipline. They took the view that "there is no contribution inherent *in* new knowledge; there is only contribution *to* some area of the use of knowledge." The authors described the Forest Service as a mission-oriented organization, with the motivation for addressing research problems developing from scientists' curiosity coupled with the needs of some nonscientific individual or group. Based on these observations, they contended that a contribution should be evaluated in terms of its relevance to the solution of real problems. The contribution is shown as progressing through three stages: (1) work product, (2) communicated product, and (3) used product. The contribution could be measured at any of these stages. Ideally, one would like to evaluate a contribution at each stage, looking for concurrence in the evaluations; however, evaluation becomes more difficult as one proceeds through the stages. The authors decided to measure the contribution of a researcher by three standards: (1) opinion of the researcher about his/her contribution, (2) events attended, and (3) written product.

Lingwood and Morris found that their standards correlated "about 0.50 with each other." They compared their ratings with those made by the assistant directors, annual performance reviews, and evaluation panels, and found that their measures correlated "...moderately with AD's ratings, and a bit less with supervisors' and panel data." They also found that there was "...not a very good convergence among the AD, supervisor, and panel data about the researcher's contributions." They concluded: "There is only modest agreement among any of the measures of contribution, and our questionnaire data at least look no worse than the information now collected to evaluate Forest Service researchers."

RESEARCHERS PRODUCTIVITY IN THE FOREST SERVICE

As is apparent even from this limited survey of the literature, there is no consensus on an appropriate measure of researcher productivity. Although extensive research has been done in a few disciplines (notably physics), there is no indication that factors and measures appropriate in one discipline are suitable for another. In terms of forestry research productivity, the literature offers many enticing areas for research but few answers to specific questions.

We have been addressing the problem of understanding and developing measures of forestry researcher productivity at the North Central Forest Experiment Station in Research Work Unit (RWU) NC-4204, Methods for Evaluating Forestry Research. Although approaches for attacking the problem are still being discussed, we began our studies by looking briefly at the number of publications produced by individuals. In the Forest Service, researchers and research managers generally agree that number of publications is the main (and at times, only) criterion for evaluation and promotion

(Lingwood and Morris 1976). Number of publications is also an interesting point of departure in the study of measures of researcher productivity because the numbers produced by individuals vary widely. As Shockley (1957) pointed out in a study of the publication rates of scientists at the Los Alamos Science Laboratory:

> In the study presented here the individuals are presumably specially selected by natural ability and specially trained to accomplish scientific production. Yet the spread in rates is enormously greater than it is for more physical activities.... If the number of scientific publications is used as a measure of productivity, it is found that some individuals create new science at a rate at least 50 times greater than others.

One can obtain an overview of the variability in researcher productivity in the USDA Forest Service by analyzing data in the Research Dissemination Database (RDD). We created the RDD to analyze the outlets used by USDA Forest Service researchers (Forest Service cooperators and employees) in disseminating research results. It contains information describing all research results published in fiscal years 1980 and 1981 (October 1, 1979 through September 30, 1981) by Forest Service researchers at eight regional experiment stations (the Forest Products Laboratory was excluded from this analysis).

To describe the variability in researcher productivity, we eliminated some entries in the RDD that were relevant for assessing outlets used to disseminate research results but cluttered the picture for this analysis. The result was a modified database containing published manuscripts reporting research results by USDA Forest Service employees in fiscal years 1980 and 1981.

During fiscal years 1980 and 1981, 1,561 Forest Service researchers senior-authored 3,621 publications. More than half (53 percent) of the publications were multiauthored (fig. 1). This is comparable to percentages reported in other research disciplines in the U.S. For example, an earlier study showed that in chemistry about 80 percent of the papers are multiauthored, in physics 60 percent, and in biological sciences 40 percent (Zuckerman 1968). However, Leikola (1981) reported that only 25 percent of Finnish forest sciences publications have more than one author.

Twenty Percent of Authors Produce 50 Percent of Publications

In our study, the average annual rate for the 2-year period was 1.2 publications. This average is low because researchers are credited only with the work they senior-author. We do not know if, in the Forest Service, the designation of an individual as a senior author symbolizes any extra credit or responsibility for the work carried out. Zuckerman (1968) identified three patterns of name-ordering on scientific papers: (1) patterns which place all authors on the same plane (such as alphabetical lists), (2) patterns which enhance the visibility of the first author, and (3) patterns which enhance the visibility of an author other than the first author (such as the last author out of order in an alphabetical list). A future study on coauthorship patterns in the Forest Service may help determine the symbolism inherent in the name-ordering of Forest Service research papers. For this study, we'll assume that the individual designated as senior author is primarily responsible for the research, and therefore our publication counts represent a measure of his or her research effort or productivity.

Although the average annual publication rate was 1.2 publications, the range in the number of publications produced by individuals varied widely (fig. 2). A majority of the researchers senior-authored only one publication during the 2-year period. However, 17 individuals senior-authored 10 or more publications. Using this frequency distribution, I grouped the researchers into three classes: low producers, intermediate producers, and high producers (fig. 3). Low producers are researchers who senior-authored one publication. Fifty-two percent of the researchers are in this group, but they accounted for only 22 percent of the publications. Researchers senior-authoring two or three publications were classified as intermediate producers. Twenty-eight percent of the researchers were intermediate producers and they senior-authored 20 percent of the publications. Only 20 percent of the researchers were high producers, but they accounted for 50 percent of the publications. Why such a range in productivity? The RDD

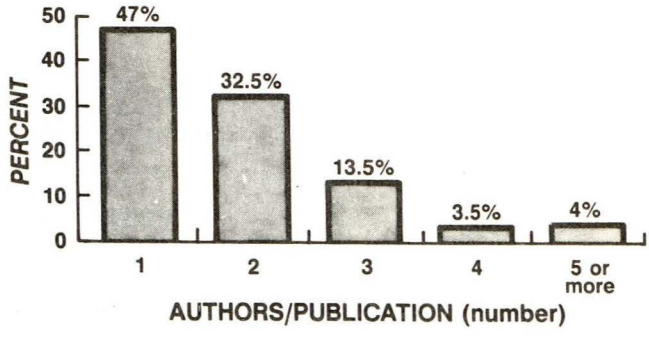

Figure 1.—*Distribution of publications by number of authors per publication, USDA Forest Service, fiscal years 1980 and 1981.*

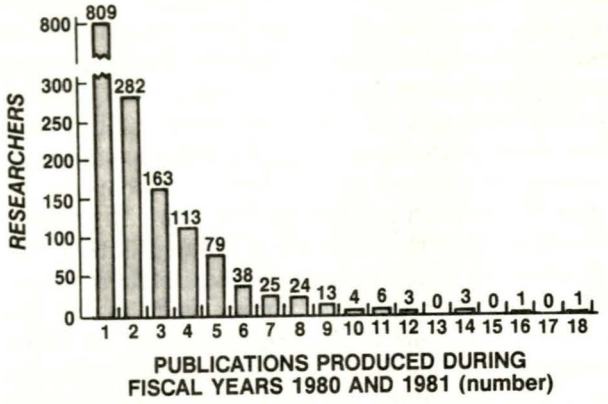

Figure 2.—*Distribution of researchers by the number of publications produced, USDA Forest Service, fiscal years 1980 and 1981.*

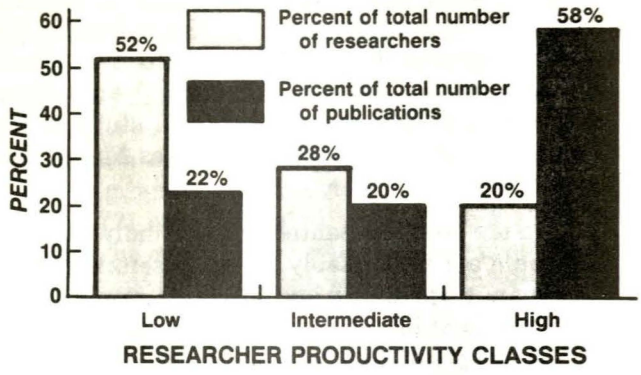

Figure 3.—*Distribution of researchers and number of publications by researcher productivity class, USDA Forest Service, fiscal years 1980 and 1981.*

doesn't provide us with the detailed information we need to fully answer the question, but we can eliminate one or two simple explanations by looking at the publication characteristics of our three researcher classes.

Why Such Variability in Researcher Productivity?

One might hypothesize that high producers had more multi-authored publications than low producers—that one reason high producers are able to produce more is that their research and writing efforts are shared among several individuals. This does not appear to be the case, however; more than half of the publications written by high producers were single-authored (fig. 4). For low producers, 36 percent of the publications had one author, significantly less than the percentage for high producers.

A second explanation of why low producers publish less than high producers might be that low producers are concentrating on quality rather than the quantity of their research output. Research cited above by Cole and Cole (1968) suggests a positive relationship between quality and quantity; however, their findings for physicists may not necessarily apply to forestry researchers. Although our database did not contain any information specifically aimed at measuring the quality of research, it did identify the outlets used by researchers to disseminate research results. The scientific prestige of the various outlets varies. There is some indication that, in the Forest Service, scientific journals are a preferred outlet because the critical peer review used in accepting articles for publication implies scientific credibility (Lingwood and Morris 1976, Simard 1984). Thus, we analyzed the type of outlets used by researchers in our three categories. We categorized the outlets as scientific or nonscientific, based on a system used by Lingwood and Morris (1976) and on guidelines published by the Forest Service for station publication series. The analysis showed there are significant differences between the types of publications used by low, intermediate, and high producers (table 1). This suggests a need for a closer look at the relationship between the quality and quantity of research produced by Forest Service scientists.

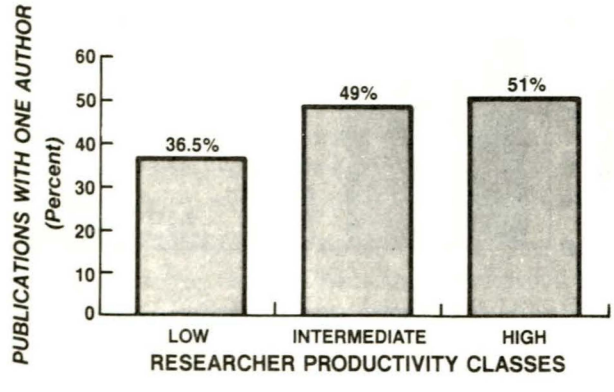

Figure 4.—*Percent of publications with one author by researcher productivity class, USDA Forest Service, fiscal years 1980 and 1981.*

One final question that can be addressed by this information concerns the distribution of low, intermediate, and high producers among research disciplines. Although the distributions are close to what one would expect given the distribution for all researchers, there are some anomalies (table 2). For example, an unusually low percentage of genetics researchers are low producers, while an unusually high percentage of hydrology and wildlife researchers are low producers. Forest inventory and analysis researchers tend to fall into the intermediate category. An unusually large percentage of insect and disease researchers are high producers.

Table 1.--Distribution of publications by type of outlet and researcher productivity class, USDA Forest Service, fiscal years 1980 and 1981

(In percent)

Type of outlet	All researchers	Productivity class		
		Low	Intermediate	High
Scientific				
Forest Service publications	22	20	24	21
Scientific journals	24	25	25	22
Proceedings	23	19	25	22
Books	6	4	5	8
Other	7	14	5	4
Total	82	82	84	79
Nonscientific				
Forest Service publications	6	6	4	8
Nonscientific journals	6	5	7	7
Other	6	7	5	6
Total	18	18	16	21
All types	100	100	100	100
Number of publications	3,621	808	1,053	1,760

Table 2.--Distribution of publications by research discipline and researcher productivity class, USDA Forest Service, fiscal years 1980 and 1981

(In percent)

Research discipline	All researchers	Productivity class		
		Low	Intermediate	High
Silviculture	19	17	18	20
Genetics	4	1	4	5
Hydrology	14	17	12	13
Wildlife	13	17	11	13
Recreation	5	4	5	5
Fire	6	7	7	5
Insects/disease	20	18	17	23
Forest products/engineering	6	6	6	6
Forest inventory and analysis	6	3	9	4
Economics	5	6	8	4
Other	2	4	3	2
All disciplines	100	100	100	100
Number of publications	3,621	808	1,053	1,760

Although the research dissemination database was not created to analyze researcher productivity, we were able to use it in analyzing some characteristics describing the productivity of individual researchers. There is indeed a wide distribution in the number of publications produced by individual researchers, and more study is needed to explain the relationships between quantity and quality of publications and overall productivity.

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SUMMARY: AN EXAMINATION OF THE IMPACTS ASSOCIATED WITH IMPLEMENTATION OF NEW TECHNOLOGY BY A MAJOR NEW INDUSTRY IN A REGION

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In 1969 the Weyerhaeuser Company acquired Dierks Forests, Incorporated. At the time of acquisition, the Dierks holdings in southeastern Oklahoma included 350,000 hectares (877×10^3 ac) of forest land, several hardwood and softwood sawmills, and a medium-density fiberboard plant. The forest land was stocked primarily with mixed stands of oak (*Quercus* spp.) and shortleaf pine (*Pinus echinata*). The silvicultural system which had been applied to the property by Dierks consisted of partial harvesting and natural regeneration.

After acquisition by Weyerhaeuser, major changes were made in primary processing facilities in southeastern Oklahoma. Those included, but were not limited to, a hardboard plant (1970), a particleboard plant (1971), a kraft paperboard plant (1971), and adoption of a softwood plywood plant (1972).

In 1971-1972, Weyerhaeuser introduced extensive loblolly pine (*Pinus taeda*) plantations to the region. The plantation system of silviculture included mechanical harvesting and intensive site preparation for regeneration. This was followed in 1981-1982 by the introduction of first generation, improved loblolly pine developed from seed sources in North Carolina.

The forested part of southeastern Oklahoma is at the northern and western edge of the southern pine region. This location is relatively isolated from the raw material flows common to the interior of the southern pine region.

A second characteristic of southeastern Oklahoma is the dominance of a single forest enterprise, which was first Dierks and after 1969 Weyerhaeuser. This includes ownership of 39 percent of the commercial forest land in the four-county area and the largest of the eight big (annual output of 3×10^6 bd ft or more) sawmills and 17 small (annual output of less than 3×10^3 bd ft) sawmills (Rudis and Jones 1981). The single pulp mill in southeastern Oklahoma is also a Weyerhaeuser mill.

These conditions of relative isolation, single major forest landowner and processor, and a history of major technological implementation make southeastern Oklahoma a unique region in which to examine the economic impact of new technology.

For these reasons, this study has been initiated with the objectives of describing the impact of new forestry technology on the economy of southeastern Oklahoma and developing hypotheses on the economic impact of new technology in forestry for testing.

The study will be conducted in three phases. The first phase will be identification of technologies implemented in the forest products industry in southeastern Oklahoma between 1959 and 1982. The macroeconomic statistics for the region will be examined to determine what correlations can be identified between the implementation of new technology and regional economic activity. The macroeconomic statistics to be examined will include, but not be limited to, gross regional product, nonagricultural employment, personal income, per capita personal income, and unemployment rate. During this phase of the study the Oklahoma State University Office of Business and Economic Research will make a significant contribution in the form of regional data and the results of their previous analyses.

The second phase of the study will concentrate on collection and analysis of cost information for the development, engineering, and implementation phases of selected technologies identified in the first phase.

The third phase will be devoted to hypothesis testing, with the aim of defining relationships between technology identification, development, engineering, and implementation.

The plan is to complete phase one by December 1985 and the remainder of the study by the end of 1988.

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SUMMARY: A TECHNOLOGY ASSESSMENT OF LOBLOLLY PINE[1]

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During the past 70 years, loblolly pine (*Pinus taeda* L.) has become the major commercial forest species of the Southeast. The introduction of numerous new production technologies has bolstered its utilization in the manufacture of paper, plywood, and other forest products, thereby increasing the demand for pine stumpage. At the same time, new technologies for the tree have had significant effects on potential total timber production available from the region. Unfortunately, there has been little research which systematically assesses and projects the impacts of these technologies. Our objective is to perform such an assessment of the technologies that affect the growth of loblolly pine.

Technology assessments of this sort typically feature impacts on many decisionmaking criteria: income, employment, energy, environment, etc. The primary assessment, however, is economic, focusing on the direct and indirect effects on price, quantity, and social welfare arising from adoption of the technology in question. Two commonly used and interrelated approaches for this assessment are the production function approach and the consumer/producer surplus approach. Both of these approaches can provide a focus for the economic analysis of technical change and their use will depend on the ultimate data availability for new technology.

The technologies examined can be divided into two overall categories: Current production technologies (i.e., biological and managerial technologies currently in use, such as tree improvement, regeneration techniques, fertilization, site modification, and insect and disease control), and secondary technologies (i.e., nonbiological, such as machinery and the dissemination of technology or uncertain emerging technologies). Technologies will be discussed separately using similar frameworks of organization to better compare their disparate effects. Direct and indirect effects will be discussed, where possible, within the economic analysis, and other nonmarket effects that address other criteria will be analyzed separately.

[1]*The USDA Forest Service, Southeastern Forest Experiment Station supported this research.*

THE MARKETING OF EVALUATION RESEARCH

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Evaluation research is a line of inquiry and practice that deals with the performance and usage of evaluations. Typical activities in the field include the development of evaluation methods, the study of factors that influence the use (or non-use) of evaluation results, and, at times, the actual carrying out of evaluations, due to the fact that such work is often research in and of itself.

The purpose of this paper is to explore the topic of evaluation research within a marketing context.

Marketing is also a line of inquiry and practice. Amazingly diverse in scope, it centers on the process of exchange between producers and consumers and is directed, primarily, at those producers who would more successfully make such exchanges. Although its origins and primary applications are found in the private sector, its concepts and basic framework for viewing problems are in use in various nonprofit and public sector organizations.

This paper's objective is to demonstrate, mostly by example, how evaluation research can fit into a marketing framework and the possible fruits of doing so. Consider the following imaginary dialogue:

> **Observer** "I just finished reading a review article on quantitative methods for selecting research projects that was in the journal 'IEEE Transactions on Engineering Management'. It said that over the past 15 to 20 years more than 100 such methods have been developed but that few, if any, research organizations have ever used them. How is it that people can continue to develop such useless junk? How do they survive anyway?"
>
> **Marketing Person** "Actually, these people are probably surviving quite well. What they produce is not junk but instead is a type of very high quality evaluation research product. What you failed to notice is that these products are not designed for research organizations but for an entirely different market, the publishers of scholarly journals who put them to very good use."

Marketing, to repeat, is about the making of exchanges and is designed to help the producer do so more effectively. To accomplish this task it adopts a "consumer orientation," which is in effect "a management orientation that holds that the key task of the organization is to determine the needs, wants, and values of a target market and to adapt the organization to delivering the desired satisfactions more effectively and efficiently than its competitors" (Kotler 1976).

The marketing approach sees four critical sets of manageable variables involved in this process once certain basic objectives have been set, strategies developed, and target markets identified and understood. These elements are referred to as the "marketing mix" and are composed of decisions with regard to products themselves, price, promotion (or communication), and distribution. Carried to its fullest extent, a particular marketing mix may be designed for each target market.

The relevance of this topic and this paper in terms of the broader effort of improving evaluation systems for forestry research is quite straightforward, given the fact that a great many workable approaches have existed and been known for evaluating forestry research, yet they have not been used in any systematic evaluation process. Down through the years *ad hoc* evaluations have been carried out and used by various administrators in the Forest Service. However, there has never been an attempt to set up a systematic evaluation system for research within the Forest Service nor in any other forestry research organization.

The point of the present paper is not to argue that evaluation is good or bad or should or should not be used by the Forest Service or other organizations. Rather, the point is that if there is some commitment

to initiating systematic forestry research evaluation in practice, then a marketing approach might well contribute to understanding what is involved, what the evaluation product can be, what "producers" and "consumers" of evaluations gain and give up in an "exchange," and how evaluations can be looked at in a pricing context. This type of perspective will provide useful background understanding which forestry administrators can use to build productive forestry evaluation systems.

EXCHANGE

Marketing is rooted in the concept of exchange, that is, that individuals and organizations gain sustenance through exchanges of tangibles and intangibles with other individuals and organizations. Exchange forms the core phenomenon for study in marketing (Bagozzi 1975).

Exchangeability is the capability of a thing (tangible or intangible) being traded for some other thing, for a price (Fine 1981). Ruling out coercion and the like, "the gist of the exchange notion is that producers and consumers will exchange product for payment if and only if both expect their net satisfaction to be higher after the transaction has been consummated" (Enis 1982).

Exchanges generally take on one of three forms (Bagozzi 1975). A "restricted exchange" involves two-party reciprocal relationships, each party receiving something of value from the other. A "generalized exchange" involves at least three parties, in which the parties do not benefit from each other directly but only indirectly. This might be represented by: A gives to B, B gives to C, and then C gives to A. The third type, a "complex exchange," involves a system of relationships among at least three parties, each of which is involved in at least one direct exchange, while the entire system is organized by a web of interconnecting relationships.

Individuals and organizations that do evaluation research work are involved in exchange relationships with other individuals and organizations. These exchanges involve a mix of tangible and intangible things flowing between those involved.

Figure 1 depicts the formal products and services, and the relationships common to evaluation research. It could represent, for example, a university professor (professors have done much of the evaluation research work in the areas of agriculture, industry, and forestry).

Professor X in this example has received a grant from a research organization to do a study on, say, the economic returns to a particular line of research. Professor X also receives a title, office, and support from his employer, the university. With the grant Professor X pays a supplier, perhaps a graduate student, in exchange for his services (help) on the study. Together, Professor X and the graduate student complete the study. They send it, in report form, off to the research organization to complete that exchange. Professor X, with or without the graduate student, may prepare one or more research manuscripts based on the study and submit them to various publishers, such as SAGE, which puts out the journal "Evaluation Research". Found satisfactory, the manuscripts are published, completing this exchange. Later, in at least partial return for his rank, office and the like, Professor X forwards evidence of his publication to his employer, the university.

In addition to identifying common reciprocal relationships in evaluation research work, figure 1 brings out various other aspects of the exchange process. The evaluation researcher functions in several distinct markets. Each market, in exchange for the evaluation researcher's offerings, pays a price but that price differs considerably from market to market. All markets, however, part with scarce items. In exchange for these "payments" the evaluation researcher has offered the results of his work, but in a somewhat different form, to each market.

PRODUCTS

Marketing is about the exchange of things between producers and consumers. Generically, all such things are considered as products, that is, anything having the ability to satisfy human needs and wants (Fine 1981). More specifically, organizations can try to market up to six types of products (Kotler 1972): goods, services, organizations, persons, places, and ideas.

Figure 1.—*Common exchange patterns in evaluation research.*

Due to its consumer orientation, however, the marketing approach requires producers to attempt to define products from the consumer's point of view. To do so leads the producers to recognize, and work with, the notions that:

1. Consumers receive psychological as well as physical benefits in making exchanges (Kinnear and Bernhardt 1983).

2. These benefits may be enhanced or diminished, depending on the consumer's evaluation of the product as part of a total package which often includes features that are not strictly necessary to enjoy the product itself.

A product's physical attributes motivate what Bagozzi (1975) calls "utilitarian exchanges," those stimulated by the consumer's anticipated use of the tangible characteristics associated with the product. In contrast, "symbolic exchanges" involve the transfer of psychological, social, or other intangible entities. This concept "...means that sellers of goods are engaged whether willfully or not, in selling symbols, as well as practical merchandise..." because "...people buy things not only for what they can do, but also for what they mean" (Levy 1959 as quoted by Bagozzi 1975). Exchanges commonly have both utilitarian and symbolic aspects.

In marketing terminology, the producer's offering is referred to as the "formal product." The satisfaction that consumers experience from the formal product is termed the "core product." It is also meaningful to delineate the "augmented product"—the total package of product features made available to the consumer—which might include, for example, the comfort of the seats in a concert hall in addition to the symphony performed. The satisfaction received by the consumer from the augmented product is sometimes referred to as the "peripheral product" (Laczniak 1980).

Evaluation researchers produce various formal products ranging from goods to services and, perhaps, even to ideas to the extent that they seek to advance the concept of evaluation itself. A few examples of such products are given in table 1.

Table 1.--Examples of evaluation research products

General example	Specific example
PRODUCT--GOODS	
Scholarly work	An article in the Journal of Forest Science titled "Economic Impacts of Structural Particleboard Research" (Bengston 1984)
Evaluation technology	A method for measuring the benefits of drought research (Leholm et al. 1981)
PRODUCT--SERVICES	
Education	A university course titled "Sociology 5821: Evaluation Research," which is to "prepare students to conduct actual evaluation research" (Office of Admissions and Records 1980)
Consulting	A multidimensional R&D benefit/cost model developed for the U.S. Dept. of Energy (Silverman 1981)
PRODUCT--IDEAS	
Concept of need	"... business firms and government agencies badly need better measures of return from R&D" (Mansfield 1965)

From a marketing standpoint these formal products become core products when viewed through the consumer's eyes. One set of consumers, figure 1, is composed of research organizations.

For public forestry research organizations, evaluations tend to be used for two general purposes (Gregersen *et al.* 1983): (1) Program planning and budgeting and (2) justification of past research (often in support of new proposals). Table 2 explains these uses in greater detail. It also shows that the final consumers of the evaluation product may or may not be those who produce or commission the product.

Disregarding, for the moment, who the final consumer is for the product, it can be said that all the formal products of evaluation research aimed at the research organization are information products of one sort or another. The information contained in evaluation reports or generated by evaluation methods are a major source, but not the only source, of the satisfaction that the consumer derives from the exchange. In Bagozzi's terms the consumer's anticipated use of this information stimulates the "utilitarian" aspects of the exchange. Appropriately, Eisgruber (1978) comments:

> Data are not information. The demand for data is generated by the need to make decisions on problems, but decision makers rarely use raw data. Rather, there are intervening acts of interpretation, through statistical and economic analysis, policy staff and political evaluation, etc., which transforms data into information by placing them in a specific problem context to give meaning to a particular decision.

In an economic sense, this decision-based demand for information stems from the uncertainty which surrounds decisions. Uncertainty, as Hirshleifer (1973) defines the term, "...is summarized by the dispersion of individuals' subjective probability (or belief) over possible states of the world." He says, further, that information "...consists of events tending to change these probability distributions...it is changes in belief distributions—a process, not a condition—that constitutes here the essence of information." But Nicholson (1972) says, "First, the individual is concerned with the utilities of uncertain events." In effect, then, it is the interaction of two factors, uncertainty and utility, that stimulates the demand for information that Eisgruber referred to and that gives potential value to evaluation information, which here is an enhancement of the expected value of the decision to be made.

From the consumer's standpoint, then, one form of utility received or anticipated as a result of the exchange with the evaluation researcher is a reduction in uncertainty. But, as the marketing approach indicates, the core product may have psychological, social, or other more or less "symbolic" values as well.

Table 2.--Evaluation users and uses in a public research organization[1/]

Evaluation producers and users	Major Purpose of Evaluation: Program planning and budgeting	Major Purpose of Evaluation: Justify past research and support new proposals
Legislators and executive	Used to evaluate proposals and supporting documentation, make specific allocations of budget, confirm intentions	Used to explain and support budget and policy decisions to electorate--rare in forestry research matters
Forestry research administrators	Used to facilitate discretionary budget and hiring decisions, enhance program management and output monitoring, check for policy compliance	Used to validate past research efforts to legislators and executive, help substantiate funding proposals
Researchers	Used to facilitate specific project funding allocations, design new projects, explore feasibility	Used to account for completed projects, provide supporting information for new projects

1/ Gregersen et al. 1983, p. 74.

The nature of the symbolic utility of evaluation research to the research organization can be speculated upon. Certainly the fact that industrial groups have funded a great deal of evaluation research, the results of which they have been unable to use (cf. Clarke 1974), might be taken to mean that they receive some other value from the endeavor. According to Rettig *et al.* (1975), one of the three contributions of research and development to the firm is that it provides the firm with an image of progressivity. It seems at least possible that evaluation research may have similar symbolic value to the research organization. According to Mansfield (1981), in some industrial laboratories quantitative evaluation models "...are little more than window dressing for professional hunches and intra-company politics."

One purpose of looking at the product from the consumer's viewpoint is, of course, so that the producer may better perform the task of product management: the planning, organization, implementation, and control of products made available to consumers. More specifically, it involves activities such as setting product goals and objectives, determining the benefits or utility that product users derive, evaluating product performance, modifying existing products, and developing new product offerings (Laczniak 1980).

The notion of the augmented product—that is, the total package of product features made available to the consumer—is a facet of product management. The association of a prestigious personality with an evaluation, for example, may have little influence on the content of the evaluation's results. Nevertheless, in the eyes of the consumer—for example, a Congressional committee—this association may enhance the evaluation's acceptance by helping to establish its objectivity. A 1976 report from a House subcommittee, for example, said, "The most significant factor in evaluating a benefit-cost study is the name of the sponsor" (Gregersen *et al.* 1983). Table 3 summarizes the product concepts discussed.

PRICE

For a marketing transaction to take place, something must be paid out by the purchaser (Fine 1981). The purchaser/consumer need not necessarily pay the producer directly, as the earlier discussion on the three general forms of exchange showed. Nevertheless, the consumer must part with something of value and, eventually, the producer must receive something of value—sometimes directly from the purchaser and sometimes indirectly as a result of the consumer's actions.

Private sector marketers typically have concentrated on the money aspects of price but, as Fine states, "If consumers do indeed perceive themselves as expending resources beyond money when they make purchases, it becomes important for purveyors to take into account these 'things' given up in exchange" (1981). To do so he develops a price construct with two components. One is the price paid in cash, the other is a nonmonetary or intrinsic component extending beyond money, which he calls social prices. In equation form the construct becomes:

$$P = p_m + \sum_{i=1}^{n} p_i$$

Table 3.--A typology of the evaluation product

	A narrow conception	A broad conception
From producer's standpoint	The formal product --the evaluation product itself (that is, the study, method etc.)	The augmented product-- all the attributes connected with the evaluation product (that is, the association of outside experts, etc.)
From consumer's standpoint	The core product-- the satisfaction consumers experience from the formal product (that is, reduction of uncertainty, image of progressivity, etc.)	The peripheral product-- the satisfaction consumers experience from the augmented product (for example, greater confidence due to objectivity of outside experts involvement in product development)

Source: Adapted from Laczniak 1980.

where:

P = total price
p_m = monetary price
p_i = a set of "social" or nonmonetary prices.

It would be difficult to apply this model, yet its concepts are useful for illustrating how consumers respond to a price construct greater than money alone. Consider an example given by Kinnear and Bernhardt (1983), which contrasts a gallon of milk for sale at $1.75 at a typical supermarket with the same item for sale at $2.05 at a 7-11 type store. Milk purchasers at 7-11 are compensated for paying more money by the convenience, such as being able to park close to the door and to avoid long waits in line at the checkout counter. At the supermarket, the situation is reversed.

The nonmonetary price in this example is, of course, the consumer's *time* which is one of four categories of resources that Fine suggests as being given up (beyond money) in exchange for product offerings. The remaining three are effort, lifestyle, and psyche.

As Fine says, expending one's *effort* in exchange for a product is merely bartering one's services in that exchange. Other things being equal, the difference between time, as a cost, and effort, would seem to be the negative or positive utilities associated with the effort—fatigue, for example—or the pleasure of doing something by one's own hands (or head).

Modification of one's *lifestyle* is also a price paid out in many forms of exchange. As Fine says, "While some may thrive on diversity and change, most individuals look upon the prospect of disruption of the status quo with at least some trepidation and this is true whether the anticipated change is for the better or for the worse." Another part of the price of exchange often amounts to a forfeit of self-esteem, identity, self-assertion, privacy, control, freedom from fear or risk, or other such losses affecting a person's peace of mind; they are thus grouped under the heading of *psyche*.

One final aspect of this price concept is that with the exception of money, each of the four categories of resources listed (time, effort, lifestyle, and psyche) might be either part of the cost of the exchange or a benefit-generating attribute of the product. Time and effort savings are often a product's major benefit, for example.

The acquisition and use of evaluation research products obviously costs the consumer. As figure 1 illustrated, these costs can take on various forms in the exchange relationships between Professor X and his publisher, the research organization, and his employer the university. From his publisher Professor X received no money; in fact, he may even have paid a page charge to the publisher. What he got from the publisher was a formal recognition of the validity and importance of his work—an intangible that he will trade with others like his employer later on.

The costs paid by the publisher on the other hand, in addition to the money involved in the actual journal printing, may involve the expenditure of considerable time and effort to judge the worth of the Professor's manuscript. Of course, it is also possible that manuscript reviewers enjoy such choices; yet, those who sit in judgment in such situations risk the loss of professional esteem should they validate what later proves to be poor work.

In addition to paying money to acquire an evaluation research product, research organizations are often faced with additional money costs in using it. One of these costs is more or less a "translation" cost. In discussing the use of research evaluations done by "experts," Fishel (1981) says, with reference to their products: "In most cases, this usually consists of a partial analysis of the total decision situation. Someone else then must further analyze the contribution of the experts...." Mansfield (1981) cites an estimate for setting up an evaluation model developed by Honeywell at about $250,000, and the annual cost of maintaining it at about $50,000.

An additional monetary aspect of the usage of evaluation research products is the distribution of the costs, and benefits, that may arise as a result of implementing an evaluation's findings, a point which Wildavsky (1978) has made.

As has been mentioned frequently here, research organizations have generally declined to adopt and put into everyday use the many formal, quantitative evaluation methods that have been developed, supposedly, for them. Most explanations of this lack of application cite technical problems with the methods—that is, the products don't work. While this may well be a major cause, there may be cost/price problems as well that go beyond the money involved.

The dominant forms of research evaluation within research organizations are managerial expertise and peer group reviews. This, Bozeman (1979) argues, is an outgrowth of "straight arrow" science, which is more or less the textbook model of science. Because most of the science administrators are former scientists, the clients and/or fundees are scientists, the outside advisors are scientists, and scientists have been instrumental in establishing and structuring the organization, the values, norms and interests of the scientific community are pervasive

and often influence nonscientists who may be involved.

Straight-arrow science, Bozeman says, purports that the work of highly specialized professionals can only be evaluated by other highly specialized professionals. It also encompasses the norm of "autonomy" which is almost universally accepted in the scientific community.

The problem here is that straight-arrow science spills over into the arena of science policy and often means that only internal, science-based criteria are employed in research assessment.

Adopting evaluation methods that include criteria other than theoretical and technical considerations may then imply to the scientist-dominated research organization significant costs that might be described in terms of "lifestyle" and "psyche" costs.

DISCUSSION

This paper has sought to show how evaluation research can be looked at within a marketing context. It demonstrates that evaluation researchers can be conceived of as producers/marketers who design and offer various sorts of products to various markets. The people who accept the evaluation researcher's wares anticipate various kinds of benefits from the products and pay various kinds of prices to acquire and use them.

Marketing is more than a framework, although its orderly conceptualization of the factors and forces involved in exchange relationships is one of its strengths. It also provides direction for the gathering of information and a means to make positive use of information in terms of product, price, promotion, and distribution decisions.

Evaluation researchers, as conceived of here, apparently do not have much in the way of marketing problems in their exchange relationships with markets like journal publishers and their employers. They do, however, have problems with another major market, research organizations. It could also be the case that research organizations, or certain elements within them, have problems with the marketing programs of evaluation researchers.

The marketing approach differs from what might be called "marketing by default" in that it makes product price, distribution, and promotional decisions by design and with forethought. One element of forethought entails the definition of the markets to be sought and at times a further segmentation of these markets into distinct groupings. Market definition is the task of choosing the arena in which to compete and the process through which demand segments within a market are defined and described and their potential as market targets assessed (Fennel 1982).

The research organization may be thought of as a market having distinct demand elements (segments) within it. One way to segment this market might be on the basis of the "roles" played by evaluation information that Fishel (1981) lists in his assessment of evaluation research related to agriculture (table 4). Evaluation researchers, therefore, perhaps have implicitly targeted Fishel's education role market segment at least in major part.

Gregersen *et al.* (1983) would have called "education" by another term, "justification." Gregersen's other major use of evaluation, program planning and budgeting, could be the market segment to which much of the often-mentioned quantitative evaluation methods were aimed. It seems clear, however, that the characteristics of this segment have not been well defined and described as a marketing approach would dictate. Part of the problem is that evaluation researchers tend to view the consumer in a manner quite different from the marketer. Fine (1981) explains:

> Marketers assume limited rationality in individuals. They are aware that consumer choice decisions are complicated phenomenon—the complex man theory—and are based on emotion as much as upon reason. In addition to reason decision making rests upon phenomena such as—perceptions, values, attitudes, group influence and personality. Marketers further believe that utility satisfaction is not ordinarily maximized but merely "satisfied," that is, satisfied with something "good enough" rather than optimality. Thus marketers investigate the properties inherent in product offerings that are capable of providing utility and then set out to provide those products most likely to satisfy.

There is ample evidence to demonstrate how evaluation researchers adopt a decidedly nonmarketing view of their potential consumers in the planning and budgeting segment. Souder (1978) does this nicely as he contrasts the "real world" with an idealized view of decisionmaking commonly assumed by the evaluation researchers (table 5).

Table 4.--Fishel's view on the success of research evaluation information in fulfilling its base roles[1/]

Role	Fishel's assessment
Education[2/]	In this role, "... we have probably achieved our singular success. The studies to date and resulting information have been good and they have been useful."
Policymaking	In this role, "... I would submit that R&E evaluation information has had little direct effect ... for whatever reasons, economic returns information is not considered useful or adequate relative to other information."
Resource allocation	"... evaluation information has not entered directly in the consideration of program or budget development decisions with which I have been associated at the federal or state levels."

1/ Fishel (1981).
2/ "primary function of making us more knowledgeable about the R&E environment ... foundation or base knowledge"

CONCLUSION

To reiterate, exchanges take place when each party involved in that exchange anticipates that his net satisfaction will be increased as a result of the exchange.

One party to the exchange process for certain lines of evaluation research products is the individuals involved in research planning and budgeting. These individuals could be coerced into making use of evaluation methods other than the manager expertise and peer group review approaches, but then there would be no exchange and probably little value resulting. To culminate free exchanges with this group requires the development of products that will be perceived as generators of net satisfaction. It is possible that some of the products that have been offered would work quite well if they were augmented to include attributes such as training programs and "after sale" consulting services.

Whatever the case, it is necessary to do some market segmentation research prior to putting marketing tools to work here. We need to know a great deal more about the consumers in question.

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Table 5.--Two views of research decisionmaking

The viewpoint that management science models seem to take
1. A single decisionmaker, in a well-behaved environment.
2. Perfect information about candidate projects and their characteristics; outputs, values and risks of candidates known and quantifiable.
3. Well-known, invariant goals.
4. Decisionmaking information is concentrated in the hands of the decisionmaker, so that he has all the information he needs to make a decision.
5. The decisionmaker is able to articulate all consequences.
6. Candidate projects are viewed as independent entities, to be individually evaluated on their own merits.
7. A single objective, usually expected value maximization or profit maximimization, is assumed and the constraints are primarily budgetary in nature.
8. The best portfolio of projects is determined on economic grounds.
9. The budget is "optimized" in a single decision.
10. One single, economically "best," overall decision is sought.
The real world environment
1. Many decisionmakers and many decision influencers, in a dynamic organizational environment.
2. Imperfect information about candidate projects and their characteristics; outputs and values of projects are difficult to specify; uncertainty accompanies all estimates.
3. Ever-changing, fuzzy goals.
4. Decisionmaking information is highly splintered and scattered piecemeal throughout the organization, with no one part of the organization having all the information needed for decisionmaking.
5. The decisionmaker is often unable or unwilling to state outcomes and consequences.
6. Candidate projects are often technically and economically interdependent.
7. There are sometimes conflicting multiple objectives and multiple constraints, and these are often noneconomic in nature.
8. Satisfactory portfolios may possess many noneconomic characteristics.
9. An iterative, recycling budget determination process is used.
10. What seems to be the "best" decision for the total organization may not be seen as best by each department or party, so that many conflicts may arise.

Source: Souder 1978.

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SUMMARY: ESTIMATING PRODUCTIVITY CHANGES DUE TO TIMBER MANAGEMENT RESEARCH IN THE LAKE STATES

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A substantial portion of the North Central Forest Experiment Station's program consists of timber management research. The effects of this research on the productivity (either potential or realized) of the Lake States forests have not been estimated.

The question of improved productivity resulting from research-generated knowledge hinges on two factors. First is the per-acre increase in growth and yield of the researched timber type and stand condition due to the new information. An example could be an increase in growth and yield due to a new thinning schedule. In the Lake States, we have recently learned that it is feasible to grow red pine at higher basal areas. The effect of this new information can be measured and the benefits estimated.

The second factor is the number of acres on which improved management is applied. Factors affecting the rate of diffusion are important determinants of the benefits to research. It can be difficult to correctly attribute the benefits from applied knowledge to the research or to technology transfer efforts.

Of course, costs of the relevant factors in both research and extension or technology transfer efforts must be accounted for.

Two approaches to estimating some of the impacts from, and returns to, timber management research in the Lake States are being considered. Not all effects can be estimated, but these studies will provide a start.

The first study will take advantage of a technology transfer effort in Itasca County. The North Central Station and the County will be implementing the latest research recommendations in timber, watershed, and wildlife management. Differences in per-acre productivity will be compared in a before-and-after format on the affected acres. Comparisons will also be made with neighboring management units. Historical records should allow us to identify the changes.

A second study under consideration is to look back at the knowledge accumulated over the past several decades in timber management research. The scheme will be to select timber types and stand conditions on which research has been done and compare recommended (or practiced) management before and after new knowledge was gained. In effect, we will be asking, "How would this stand have been managed if we had 1950 knowledge versus 1984 knowledge?" The management recommendations can be run through a computer model such as STEMS, and the difference quantified.

SUPPLY FUNCTIONS AND SOCIAL RETURNS TO RESEARCH

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Two major approaches are currently used by economists to study the economic impact of new process research which raises the productivity of factors of production (for reviews, see Davis 1981, and Norton and Davis 1981). One approach uses supply and demand curves to estimate increases in consumer and producer surplus or, alternatively, to measure net economic benefit, which then allows the calculation of an internal rate of return on research and development (R&D) investment and benefit/cost ratios. I will refer to this type of study as a supply function approach.

The other type of study involves the estimation of a production function where research efforts or expenditures enter as a factor of production. This approach is useful in examining the impact of research in terms of its marginal productivity and may be used to determine the value of the marginal product of research or a marginal internal rate of return on research investment. This type of study is often referred to as a production function approach.

The production function approach is currently favored as being more representative of returns to research, not because it allows the estimation to proceed in concentrated industries where a supply curve may not be defined, but due to the manner in which the supply function approach has been applied. Griliches (1979) writes that supply function studies are "very data- and time-expensive and are always subject to attack as not being representative since they tend to concentrate on prominent and successful innovations and fields."

In fact, Griliches' criticism is very true of supply function studies. This stems from the fact that the supply function approach began as (and continues to be) a residual approach. There has been no effort to pin down unbiased estimates of the impact of R&D on the supply curve. All observed welfare improvements have been attributed to R&D. (For an extensive critique, see Seldon 1983.)

To make the problems more obvious, consider how such a study might proceed. We begin with observations of price and quantity at two points in time—once before and once after a particular innovation occurs. That is, all we know so far are points A and B in figure 1. The next step is to obtain an estimate of the price elasticity of supply. This is usually obtained from other studies which do not take R&D into consideration. We then construct supply curves through the two points. In figure 1, supply curve S_A is the presumed supply curve before the innovation, while supply curve S_B is presumed after the innovation. A demand curve D is then drawn through the two points. This graph is then used to calculate changes in consumer and producer surplus and net economic benefits. All observed societal improvement is then credited to R&D.

This procedure introduces various biases into the estimates. For instance, the rightward shift of the supply curve may have been caused in part by a fall in the real wage. Then the shift attributed to R&D is overestimated. Alternatively, perhaps the real wage increased. Then the impact of R&D is underestimated.

The elasticities of supply and demand may also cause problems. If R&D is indeed important but has not been used as an exogenous variable in the estimation of the supply equation then the elasticity of

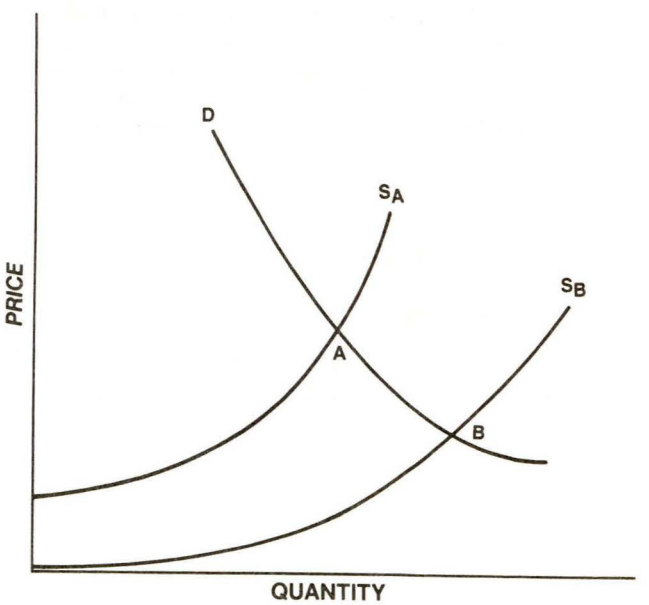


Figure 1.—*The conventional supply function approach.*

supply is likely to be biased, although the direction is uncertain. In the application described above, the demand elasticity is almost certainly biased, since there is no reason to presume the demand curve has not shifted.

Another troublesome aspect of this type of study is determining the costs of research which allow a particular breakthrough. Any innovation is accomplished by building upon past achievements. Therefore, questions arise: How far into the past should we count costs? And how can we account for all recent costs that allowed an innovation? Surely there are spillovers such that one type of research aimed at a particular product aids other research directed at the same product.

After such decisions are made, benefit/cost ratios and internal rates of return may be calculated in the usual way. But another problem arises. How long will benefits last? It is customary to obtain limits on the returns to research by alternatively assuming benefits last only one period or that they will last forever.

PROCEDURE

An alternative to this residual supply function approach is a nonresidual approach which is a straightforward extension of the production function approach (this is fully developed in Seldon 1985). The production function approach was originally a residual approach, but has evolved into studies which consider R&D to be an integral part of the production process (see, e.g. Griliches 1964). Since the main concern of such studies is the impacts of R&D, issues concerning the functional form of the production function are largely ignored as not being very crucial or interesting, and the easily handled Cobb-Douglas function is commonly used (Griliches 1979).

Thus we begin with an equation of the form

$$Q_t = AL_t^c K_t^f T_t \ ,$$

where Q_t is quantity produced,
L_t is labor input,
K_t is capital services,
and T_t is an index of technology,

all at time t. Technology is a function of all past R&D effort, and may be specified as a linear function, but in some ways is easier to handle if we assume a multiplicative version:

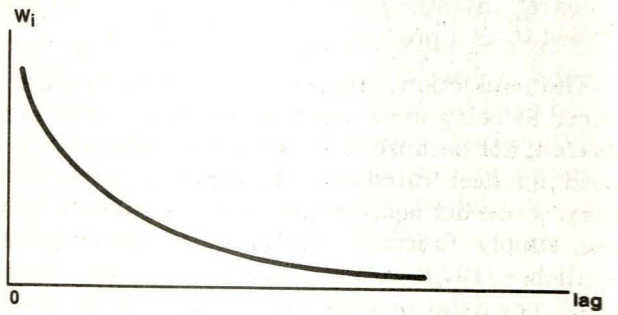
$$T = \prod_{i=0}^{\infty} R_{t-1}^{d_i} \ .$$

It simplifies the analysis to assume some form of distributed lag on the technology function. The form of the distributed lag depends upon reasonable assumptions made in light of the particular industry. We might expect that the effect of R&D "long ago" is less important for production today than more recent R&D, since the effects of R&D depreciate in a sense—perhaps because producers forget, but more importantly because new innovations displace older innovations when they both perform relatively similar functions. On the other hand, when innovations require large capital investments, we might expect that all producers will not use new processes immediately, but rather will wait until old equipment is to be replaced. The lag weights for two cases are illustrated in figure 2.

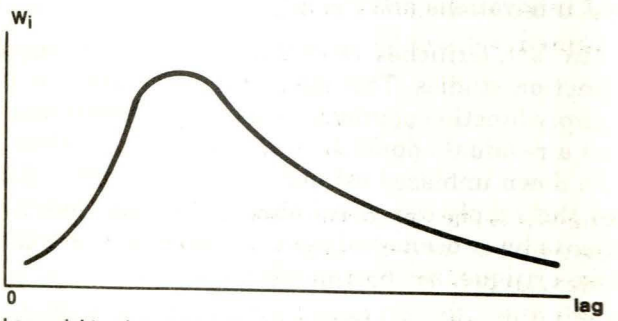
In the agricultural literature especially, it is typical to assume the form of figure 2A, which is the familiar Koyck lag. Say, for instance, that it takes 2 years for producers to evaluate innovations and then to adopt them, but all producers do so simultaneously. We assume that all firms maximize profit over a convex profit function so that industry profit is maximized (Debreu 1959). The functional form of the industry profit function is, in the Cobb-Douglas case,

$$\Pi = P_t AL_t^c K_t^f R_{t-2}^d R_{t-3}^{Ld} R_{t-4}^{L^2 d} \times \ldots - W_t L_t - I_t K_t$$

where W_t and I_t are the real wage and real cost of capital at time t. Maximizing this function with respect to L_t and K_t and solving for the optimal quan-



A. Lag weights when all producers use innovations almost immediately.



B. Lag weights when producers adopt innovations over time as their old equipment deteriorates.

Figure 2.—*Distributed R&D lags.*

tities of labor and capital services allows us to reformulate Π in terms of wages and cost of capital. Rearranging terms then gives a supply function which is linear in the logs. The research term falls through into the supply equation as an exogenous term, since any change in research effort today will not impact production until (in this case) 2 years into the future.

Using this derived supply equation and performing the Koyck transformation (Kmenta 1971) gives

$$q_t = (1 - L)b_o + b_1p_t - Lb_1p_{t-1} - b_2w_t + Lb_2w_{t-1}$$

$$- b_3i_t + Lb_3i_{t-1} + dr_{t-2} + Lq_{t-1} \ ;$$

where lower case letters indicate logs and the b_i's are nonlinear combinations of the parameters from the profit function. We can then estimate this supply equation simultaneously with a demand function, e.g.,

$$q_t = a_o - a_1p_t + a_2y_t \ ,$$

where y_t is an exogenous demand shifter.

In this manner, we can estimate the average impact of R&D on the supply equation. We can proceed in the following manner: For each period in the time series we know price and quantity and have presumably unbiased estimates of the elasticities of supply and demand (b_1 and a_1, respectively). We estimate the impact of today's R&D on supply curves in the future, *ceteris paribus*. That is, the demand curve remains the same and the supply curve shifts due to R&D *only*. In our example, the initial impact is 2 years in the future; hence, assuming the initial supply curve S_t in figure 3, we obtain the supply curve S_{t+2}. This gives us the economic impact of today's R&D that will be obtained 2 years hence, *ceteris paribus*. We then do the same for the expected supply curve 3 years hence, given the supply curve and R&D today—S_{t+3} in figure 3—and obtain the returns to research due to R&D today. We continue to do this until the benefits are small enough to be ignored. Note that this procedure is similar to the estimation of an exogenous shock in any well-balanced system. We can then obtain the total discounted benefits for research at time t as

$$B_t = \sum_{i=2}^{N} \frac{1}{(1 + \rho)^i} B_i$$

where ρ is the appropriate social discount rate, B_i is the benefits for period i years hence due to research today, and N is the time at which returns become negligible. Net economic benefit is found by subtracting the costs of research from B_t, and the benefit/cost ratio is found as usual.

We can do this for each period in our sample and then, discounting the discounted terms, we can estimate the return to the research program over the period of our sample. We may then solve for the internal rate of return to research.

RESULTS

This methodology has been applied to determine the productivity of public research in the softwood plywood (SWPW) industry (for complete details see Seldon 1985). The actual model is similar to the one described above—it has a Cobb-Douglas form, with a Koyck lag identical to the one already described. In terms of the parameters necessary for the estimation of returns to research, we estimated a demand elasticity (a_1 above) of 2.665 in absolute value, a supply elasticity of 0.5011, and L and d of the supply equation as 0.8679 and 0.0219, respectively. Costs were estimated from records of government scientists' effort in attainment reports maintained by the Forest Products Laboratory (FPL) in Madison, Wisconsin. To capture private expenditures induced by government expenditures, we multiplied the total government expenditures by the number of plants times 0.26. This factor was estimated in the case of the powered back up roll and is likely to be high relative to other innovations during the period of our study (Seldon 1985). We should, therefore, be overestimating costs and underestimating net benefits.

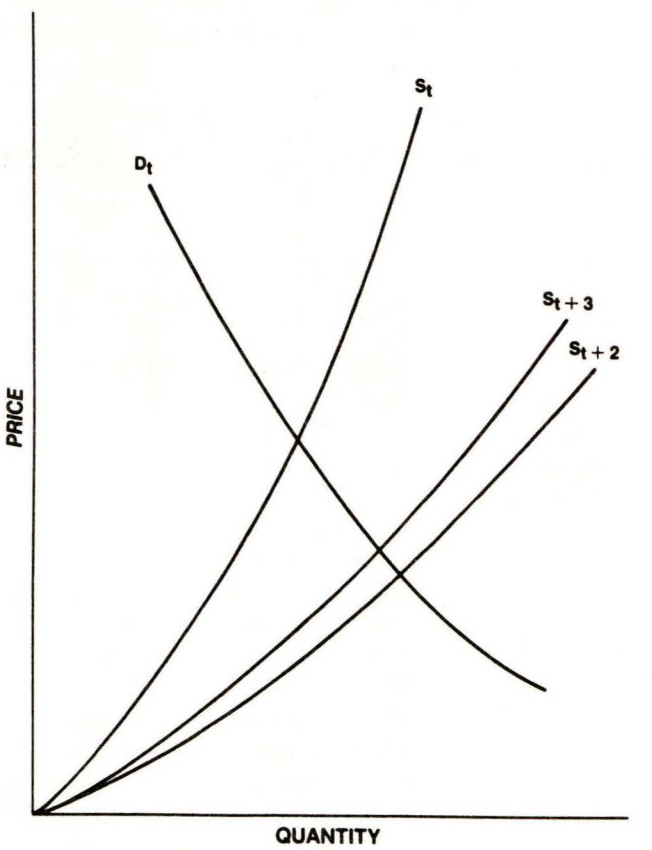


Figure 3.—*Applying the nonresidual supply function approach.*

We solved for the present value of the program for the years 1950 through 1980 assuming social discount rates of 4, 7, and 10 percent, which are the rates used for project evaluation by the U.S. Forest Service, the Water Resources Council, and the Office of Management and Budget, respectively. We then solved for the internal rate of return.

Before giving our results, we should note that intermediate results give an average value of the marginal product of research that matches rather well with those found by Griliches (1964) for aggregate agricultural research and Peterson (1967) for poultry research. In 1967 dollars, we found a first relevant lag value of marginal product of $20.92, while Griliches (1964) found $16.52 for aggregate agricultural research and Peterson found $21.09 for poultry research (for a more extensive discussion, see Seldon 1985). Our estimate was close to the higher of the agricultural estimates, and this may be expected since SWPW research was applied in nature over the period used in this sample and could therefore be more easily adopted by producers. In addition, information is passed to *all* SWPW firms much more quickly than information is passed to *all* farmers.

The results of our estimation procedure are found in table 1. Here PV indicates present value of total benefits unadjusted for costs, NPV indicates net present values, BC indicates benefit-cost ratios, while subscripts CS, PS, and NEB mean consumer surplus, producer surplus, and net economic benefit, respectively. PV_{ET} is the present value of all expenditures. All values are in 1967 dollars.

According to our calculations, public investments in SWPW research were very productive, exhibiting an internal rate of return of 422 percent. Both producers and consumers benefited from the investments, although producers tended to gain somewhat more than consumers.

Our estimate of the internal rate of return to SWPW is higher than returns calculated in the agricultural economics literature, where rates of 90 to 110 percent are on the high side (although not uncommon—see Ruttan 1980). That the estimate would be larger is not surprising, given the applied nature of the research and the speed with which the information is passed to the private sector. An alternative model which assumes that public R&D in SWPW for 1972-1980 was qualitatively different from research through 1971 did not give very satisfactory statistical results; but applying those results for the period 1950 through 1971 still gives a high internal rate of return of 407 percent.

Table 1.--Returns to public research: nonresidual supply function estimates for the 1950-1980 program assuming a multiplier of .26

Productivity measures[1/]	Social discount rate[2/]		
	.04	.07	.10
PV_{CS_T}	2,752,501,319.35	1,391,652,548.17	770,454,508.96
PV_{PS_T}	3,053,037,570.26	1,543,602,353.41	854,577,814.55
PV_{NEB_T}	5,805,538,889.61	2,935,254,901.57	1,625,032,323.51
NPV_{CS_T}	2,722,232,580.56	1,371,274,900.45	755,988,826.17
NPV_{PS_T}	3,022,768,831.48	1,523,224,705.69	840,112,131.75
NPV_{NEB_T}	5,775,270,150.83	2,914,877,253.85	1,610,566,640.72
BC_{CS_T}	87.44/1	63.83/1	48.42/1
BC_{PS_T}	96.98/1	70.79/1	53.71/1
BC_{NEB_T}	184.42/1	134.62/1	102.12/1
PV_{E_T}	31,479,488.71	21,804,082.72	15,912,250.84

1/ PV = present value of total benefits unadjusted for costs
NPV = not present values
BC = benefit/cost ratios
Subscripts
CS = consumer surplus
PS = producer surplus
NEB = net economic benefit
PV_{E_T} = present value of all expenditures

2/ Internal rate of return: 422 percent

SUMMARY AND CONCLUSION

In this paper we have developed a nonresidual supply function approach for estimating returns to research. This method is a straightforward extension of the production function approach commonly used in investigations of the marginal productivity of research. The method uses past R&D as an exogenous shift variable for the supply equation and can therefore be used to estimate returns to research under *ceteris paribus* conditions where a supply curve can be assumed.

While the approach is as data-intensive as the production function approach, the variables are usually more conveniently available than those needed to estimate a production function, and we can get all the information that the production function provides by estimating the production function parameters directly from the derived supply equation. Furthermore, this nonresidual supply function approach will credit research with retarding an inward shift of the supply curve due to, perhaps, a rising real wage rate or cost of capital. The residual approach could not be used under these conditions.

When we applied this methodology to public research in SWPW production over the period 1950 through 1980, we found that the internal rate of return to public research was high—422 percent or better. Consumers and producers benefitted, although producers tended to gain more surplus through public research than did consumers.

That the internal rate of return to SWPW research is higher than commonly found in agricultural studies is not surprising. Public research in SWPW over this period was applied research that was quickly adopted by SWPW producers. In contrast, agricultural research is often more basic, requiring years of research to obtain results that can then only be disseminated to the users over a fairly long period through extension efforts. Thus, costs are incurred for any particular project over a rather long time horizon, resulting in relatively high costs and highly discounted benefits.

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SUMMARY: PRODUCTIVITY IMPACTS OF TREE IMPROVEMENT RESEARCH IN THE NORTH CENTRAL REGION

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The North Central Region Project NC-99, "Forest Tree Improvement—Selection, Breeding and Seed Production," was initiated in 1960 and is currently in its fifth phase. Administered principally by the 12 State agricultural experiment stations in the North Central region, the project also involves formal and informal cooperative research ties with two U.S. Forest Service Research Laboratories, the Ohio Agricultural Research Service Station, Southern Illinois University, State agencies, industry, and neighboring Canadian provinces. The NC-99 project is generally perceived as a successful example of cooperative tree improvement research. By pooling their efforts, members of the cooperative have been able to undertake a greater variety of research and to conduct it more effectively than if they had worked alone. Over the past two decades, the NC-99 project has conducted provenance tests of 40 species, of which 13 are currently ready for use in establishing genetically superior seed orchards.

The overall goal of this study is to evaluate the impacts of NC-99 research and the application of knowledge generated by that research. This task is complicated by the wide variety of species and physical characteristics addressed by the participating researchers, and by the basic as well as applied nature of tree-breeding research. In addition, while the applied research results tend to be specific to the region, the results of basic research on tree biology are often of a more general nature.

A first step in the study will be to determine the cumulative investment in NC-99 research since 1960. Quantification of research benefits in economic terms will, for reasons outlined above, concentrate primarily on productivity gains realized from production of genetically superior trees. This comparison of costs and benefits is expected to be similar to previous analyses of tree improvement programs for single species. It is commonly referred to as an *ex ante* benefit/cost study. The *ex ante* approach is necessary because the genetics studies and tree improvement programs have not progressed to the point where historical performance data are available.

Evaluation of the productivity of the basic research component of the NC-99 project is expected to rely on a "partial indicators" measure. This approach has merit when attempting to measure the contribution to knowledge of a research group, such as the NC-99 project. It is most useful for gauging the output of one research laboratory or project relative to others working on similar problems. It is not intended as a procedure for measuring the productivity of individual scientists. Variables in the set of "partial indicators" might include number of scientists trained, publications, citation patterns, coauthorship patterns, and peer evaluations. While the results of this approach are qualitative, they do provide research administrators with an indication of scientific performance.

RETURNS TO INVESTMENT IN FORESTRY RESEARCH: THE CASE OF CONTAINERIZED FOREST TREE SEEDLINGS[1]

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There is extensive evidence that research has a significant effect on increases in productivity, which in turn has a positive effect on economic growth and development. A substantial amount of evaluation effort has been devoted to agricultural research and extension to measure these gains, but comparatively little has been done in forestry. It is likely that research evaluations and other forms of technology assessments will play an increasingly important role in the future of publicly supported agricultural and forestry research due to a recent court case (California Rural Legal Assistance (CRLA) and Public Advocates, Inc. vs. University of California) on the West Coast (Kendrick 1984). Evidence of this heightened interest in evaluating forestry research directions, methods, results, and impacts is provided by this workshop.

This study analyzes the economic impacts of research which led to the adoption of containerized forest tree seedlings (CFTS) in this country as a method of forest regeneration. A containerized seedling is a tree grown in a container, under controlled environmental conditions, in a relatively short period. This regeneration technique is contrasted to bareroot seedling production, where seedlings are grown in exposed seedbeds using specialized farming practices, removed from the soil and shipped to the planting site with the roots bare (Tinus and McDonald 1979).

RESEARCH BENEFITS

Economic benefits resulting from the development and utilization of CFTS technology come from the lowered cost of producing established seedlings, and employment effects, among others. This study considers only benefits measurable in terms of lower costs of production and hence consumer prices, and those resulting from a more flexible operation and the extension of the planting season.

The economic surplus model adapted to evaluate CFTS research benefits follows that of Griliches (1958). The theory behind this approach is that the adoption of technological innovations resulting from research and development reduces the marginal cost of production, increasing production and lowering the market price. The gross annual research benefit (B*) was calculated using the formula:

$$B^* = kP_{br}Q_{cs}\,(1-1/2kn)\ ,$$

where k is a percentage measure of the price discount of CFTS, or $(P_{br} - P_{cs})/P_{br}$; P is the cost of seedlings (measured per thousand), Q_{cs} is the estimated quantity of CFTS produced annually, and n is the absolute value of the price elasticity of demand for CFTS.

Values used to determine the variable k are based on bareroot and CFTS price differences in the form:

$$P = [(PR + TH + SP)/SL]$$

where PR represents production costs, TH transportation/handling costs, and SP site preparation and planting costs, and where SL is the survival rate following the model developed by Colby and Lewis (1973).

RESEARCH COSTS

Economic costs resulting from CFTS research are direct costs (e.g., salaries and supplies), indirect costs (e.g., staff training and management, opportunity costs), and complementary costs (formal and informal technology transfer costs).

Public and private agencies have supported and conducted CFTS research in the U.S. since approxi-

[1]*Research supported by the College of Forestry and Agricultural Experiment Station, University of Minnesota and the USDA Forest Service, North Central Forest Experiment Station, and Cooperative State Research Service.*

mately 1969. Costs associated with this effort are estimated following the methodology developed by Bengston (1984), who used screened publication counts as an indicator of public sector research activity combined with information about cost per scientist year, industry estimates of private sector research, and deflation with a cost of research index. This technique is used because the scope and diffuse nature of the CFTS research effort precludes the use of financial records to account for costs, and published government data are not available.

RATE OF RETURN

The internal rate of return to investment in CFTS research can be readily calculated once benefits and costs are estimated. This measure gives an indication of the highest interest rate at which money could be borrowed to finance the total research budget and still not incur a loss on the investment. Rates of return were found ranging from 37.3 to 111.2 percent under different assumptions concerning research benefits and costs (table 1).

The internal rate of return proved to be very sensitive to the price discount of CFTS (k) estimate that is used, and also to estimates of research cost levels (varied plus and minus 33 percent). Varying projections of the estimated quantity of CFTS (Q_{cs}) produced in the future did not significantly alter the results, though this may be due to the fact that the two projections made were not widely divergent.

Research evaluations such as this can supply background information, provide support for decisions already made (or provide input into changing decisions, if results so indicate), and give verification or refutation of ideas (Gregersen 1983). Elmer Staats, former Comptroller General of the United States notes: "In the final analysis, the hard choices will be political choices, but political leaders and the public need increased awareness of the arguments, pro and con, concerning the choices that must be made. Those who want to do right should at least be equipped with good information" (Staats 1974).

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Table 1.--Internal rate of return to containerized forest tree seedling research under different assumptions concerning research benefits and costs[1/]

Research costs[2/]	Case A[3/]		Case B[4/]	
	k = Upper [5/]	k = Lower [6/]	k = Upper [5/]	k = Lower [6/]
X	89.2%	54.2%	89.1%	53.8%
+33%	74.5%	38.4%	74.4%	37.3%
-33%	111.2%	74.7%	111.2%	74.6%

1/ Combining public and private containerized forest tree seedling research.

2/ Research costs: X: mean research costs estimates; +33%: upper level research cost assumptions; -33%: lower level research cost assumptions.

3/ Upper level Q_{CS} assumptions.

4/ Lower level Q_{CS} assumptions.

5/ Mean price discount of containerized forest tree seedlings (substitute: bareroot seedlings).

6/ 1/2 k.

FUTURE AREAS OF RESEARCH

THE FUTURE DIRECTION OF FORESTRY RESEARCH EVALUATION EFFORTS

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Science is dynamic. It is evolutionary in nature, building upon what has gone on before. New ideas, new hypotheses, are the mutations of scientific knowledge, spurring development in unforseeable directions. Lines of scientific inquiry are born, they flourish, they die. They evolve into new lines of science. We can forecast promising areas of development in science, for the near future, but we cannot forecast the future of scientific knowledge over the long run. Fields of scientific knowledge continuously evolve, although often not along neat, orderly lines. Nevertheless, a knowledge of where we have come from, and where we are now, can help us estimate where we may go in the future.

In many fields of science, the development of research activities tends to follow a distinctive pattern. A new, relatively unexplored field of science may first go through a descriptive phase for a period of time, where scientists are primarily observing and reporting on case histories or on the results of particular experiments. This phase builds up a rich body of literature about individual occurrences. At some point, scientists begin trying to explain these observations within a more general theoretical framework.

This development of new theory is often based upon a leap of imagination and faith on the part of one individual or a small group of people.

A period of testing competing hypotheses follows, until some theory is generally accepted by the profession. This may take a generation or more. This new accepted theory becomes the doctrine of the field, and is taught to young, incoming scientists as part of the accepted knowledge base of the field. These new scientists, steeped in the new paradigm, are able to fully exploit this new organization of knowledge. Progress within a field is often rapid at this stage. Much of the research conducted during this stage fills gaps in existing knowledge by applying generally accepted methods and techniques to new problems or new areas, or modifying existing methods and techniques to improve on solutions to problems. But rarely are the accepted problems challenged.

Thomas Kuhn (1970) and others have traced through scenarios of periodic revolutions in science. At any one time in a field of science there is a generally accepted body of data, theories, and explanations of how things are. Anomalies arise in applying accepted theories to explain reported observations. New theoretical constructions are proposed to explain these anomalies. Often these new theories are radical departures from accepted theory, and can be termed revolutions. When such a theory is advanced it is often greeted with ridicule and outright opposition by the affirmed leaders of the profession. The term "revolution" is often particularly apt because the development of a new theory or school of thought within a profession can create some real conflicts among members of the profession. The history of science is filled with stories of the divisive influence of new ideas within a field of knowledge. Professional reputations are made and broken in this battle over competing theories.

What has this got to do with a discussion of the future direction of forestry research evaluation? Well, we can ask ourselves, "In what stage of scientific development is research evaluation at this time?" And, in particular, we can ask, "In what stage of development is forestry research evaluation?" Answering this may help us to forecast its future development.

Is there a well-developed general science of research evaluation? I think not. When we look at the literature, we find many people in many fields hard at work conducting research on research, each examining a particular aspect of scientific research using particular tools and techniques developed within their discipline. Research evaluation seems to be a relatively new, fragmented area of science that shows several promising areas of research. Several independent traditions of scientific inquiry have evolved over the last decade. I won't take time to enumerate them here. I have reported on them elsewhere (for example, Lundgren 1983). If you are interested I can provide you with an annotated bibliography that explores the literature in many of these fields.

There is a lot of interesting work being done in forestry research evaluation, but I have not yet seen a definitive, comprehensive statement of the research evaluation problem, let alone a synthetic theory of research evaluation. I believe that it is something we can look forward to during the next decade or two. At least, I hope that someone can bring order into this fascinating field of inquiry. This presents a real challenge to all who are interested in the advancement of this science of science—this research on research.

The history of scientific ideas and technologies teaches us that whatever the future brings, there will undoubtedly be revolutionary new ideas and new technologies that we cannot foresee. There will be a number of surprises. We all are to some extent prisoners of our own ideas, our own images of the world. It is difficult, if not impossible, to foresee the revolutions in thought that are likely to occur in a given field. The small changes are not too difficult to forecast, but the big, revolutionary changes cannot be forecast. This is especially true if we are trying to forecast within our own field.

Those who are at the cutting edge of scientific knowledge can forecast how the most recent developments in thought within their field are likely to evolve in the near future, and how they might be used. But the true revolutions that may occur are difficult to imagine.

Research evaluation in forestry is a relatively new field. It is just now getting underway. Although there have been a few isolated studies over the past two decades, and some of these were good studies, until a few years ago there had been relatively little systematic effort to develop and apply evaluation techniques to the research process in forestry. We have heard about some of these newer studies this afternoon. But overall, the field of forestry research evaluation is going through the painful process of trying to identify what it is and what it should be doing, and trying to get organized to get on with the job.

Some may look upon this as a problem, others as an opportunity. Agricultural research has a rich tradition of research evaluation through the work of agricultural economists. Norton and Davis (1981) have recently reviewed work in this field. There has been a logical evolution of research and, as we are well aware, some of the leading efforts have been carried out here at the University of Minnesota's Department of Agricultural and Applied Economics.

Yet, perhaps we shouldn't be too envious of those doing research in agricultural research evaluation. In an established field of science, with traditional approaches to a problem, it is sometimes difficult to depart from traditional concerns and take a new look at problems in the field.

In forestry research evaluation we haven't established any traditional line of inquiry. This offers some exciting opportunities. We can take a fresh look at the research evaluation problem, building upon what has been done in agricultural economics and in many other fields as well.

I would like to see forestry research evaluation take a broad look at the entire research evaluation problem. Let's not confine ourselves to a narrow scope of work too soon, but explore the rich array of problems of interest to many different people in the research system—the funders of research, managers of research, doers of research, disseminators of research, and users of research, to name a few. Each of these groups has its own concerns about the research process. Addressing each concern may require different approaches.

During these early years of research on forestry research evaluation, I would like to see a sizable effort devoted to clearly identifying the major problems within forestry research evaluation and setting priorities on the issues uncovered. We have relatively limited resources to devote to this task. It is important that we allocate these scarce resources efficiently and effectively and justify our own research on research. It is worth keeping in mind an observation by Richard Bradfield that Vernon Ruttan quotes in his recent book, "Agricultural Research Policy" (Ruttan 1982), and that Vern repeated yesterday:

> There are many interesting research problems.
>
> Some of them are important.

There are many interesting problems in forestry research evaluation. But we need to identify the important problems as best we can, and concentrate our efforts on solving these. That is a lot easier to say than to do. But that presents a worthwhile challenge to all of us.

We won't insist that our panelists today provide us with insights as to future revolutions in thought in forestry research evaluation. But we are fortunate in having with us today two people who are at the cutting edge of research evaluation, and who can discuss some of the evolutions in thought that are likely to occur in the near future.

The first panelist is Dr. Burt Sundquist, whose name and achievements are familiar to everyone who has explored the literature on agricultural research evaluation. Dr. Sundquist is Professor in the

Department of Agricultural and Applied Economics here at the University of Minnesota. Some of his most recent innovative achievements have been in the exciting area of technology assessment (Sundquist *et al.* 1982b). If you haven't seen the trail-breaking work on technology assessment of commercial corn production in the U.S. by him and his colleagues, I urge you to look it over (Sundquist *et al.* 1982a). Dr. Sundquist is going to discuss the future of technology assessments as a methodology for evaluating research.

I believe that technology assessment is one of the most promising areas of research evaluation in forestry. To justify research expenditures in the future we are going to have to do a better job of estimating the direct and indirect impacts of research on society. It is not enough to look only at the aggregate costs and benefits of research. Some of the really critical issues, the important political ones, are the questions: Who pays for the research? Who bears the adverse impacts of implementing a new technology? Who benefits from the new technology? What effects will this new technology have on our natural resource base and on our environmental systems? These are some of the important issues addressed by technology assessment.

We are fortunate indeed to have with us Dr. Burt Sundquist to discuss the "Future of Technology Assessments."

The second member of the panel is Dr. Robert Buckman, Deputy Chief of the USDA Forest Service in charge of research programs. As head of the largest forestry research organization in the world, Dr. Buckman has played a key role in developing an interest in forestry research evaluation in the U.S. It was through his initiative that the Forest Service a few years ago began a national effort to develop methods for evaluating forestry research, a program that Chris Risbrudt now heads. Dr. Buckman also is Vice President of IUFRO, the International Union of Forestry Research Organizations, for the period 1981 to 1986. As an officer of IUFRO, he has instituted some major new initiatives to strengthen forestry research among the developing nations of the world. He will use his extensive background in research administration to explore with us some of the future directions of forestry research evaluation.

As a research administrator, one of the questions that Dr. Buckman undoubtedly must ask himself is, "Just how much effort should we put into research evaluation?" After all, research evaluation is not a free good. It would be well to ask ourselves, "What are the costs and benefits of forestry research evaluation?" Research evaluators must also justify their requests for funds. That presents an interesting challenge for the future. How can we evaluate the evaluators? And who will evaluate the evaluators? I am reminded of that famous bit of poetry by Jonathan Swift:

> So, naturalists observe, a flea
> Hath smaller fleas that on him prey;
> And these have smaller still to bite 'em;
> And so proceed *ad infinitum*."

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THE FUTURE OF TECHNOLOGY ASSESSMENTS

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When one views technology assessment in an evaluative kind of mode, clearly one is starting out with the presumption there is some strong linkage between research and development investments and activities and the generation of technology. Some of those linkages are fuzzy and indirect, and it is hard to draw direct lines in some cases between research investments and the technology that flows out of them. Yet, if we are talking about the production and use of technology as such, but in the context of research evaluation, it is that very linkage we assume is an important one. I come at this whole area of research evaluation and technology evaluation from a pragmatic viewpoint. We are not going to conduct one big evaluative effort that is going to answer all questions or deal with all problems. Rather, we are in a situation where the need is to surround questions with as much information as we can, and then try to draw some inferences from that set of information. I would certainly say that there are a variety of evaluative tools that can contribute very substantially. Technology assessment, as such, might be thought of as only one of them, and yet in another context almost any kind of evaluative effort, regardless of what kind of analytical methods or framework it uses, comes under the heading of technology assessment. By that I mean that I see technology assessment as a very broad kind of umbrella.

I like to use a broad and boundless definition of technology assessment. Technology assessment is the formal, systematic, interdisciplinary examination of an existing, newly emerging or prospective technology with the objective of identifying and estimating first and second order costs and consequences, over time, in terms of the economic, social, demographic, environmental, legal, political, institutional and other possible impacts of the technology, including those consequences which may not have been anticipated, intended, or desired by the inventors, and of specifying the full range of alternative courses of action for managing, modifying, or monitoring the effects of the technology. It is pretty hard to envision something in an evaluative mode that doesn't fit under this definition in one way or another. The other thing I think that one ought to get out of this definition is that technology assessment per se does not dictate to the analyst what kind of analytical technique to use in the process. It might well be that in the conduct of a technology assessment effort one could, at different stages, use all the analytical techniques available to the economist, from regression analysis to optimizing analytical tools, to simulation techniques, and so forth. I want to suggest at the outset that in using a technology assessment framework we are not really locked into any specific kind of analytical device. Basically, I think that is the way inquiry ought to proceed. One ought not to start out with an analytical technique and then try to apply it to some question whether or not it fits. I think one ought to start out with a broader problem setting and then move to sort out the appropriate analytical techniques to use.

Finally, as a word of introduction, one of the real dangers of the technology assessment framework is that it is so broad and general that one can end up not having gotten down to the nitty-gritty of performing specific analyses. To some extent I feel a little guilty of that in some parts of our corn technology assessment.

Let me describe a framework that we tried to use in looking at a variety of technologies that impacted on the corn production sector (table 1). As people here have suggested, one of the obvious problems is that individual technologies are not spawned independently of what is going on in the whole system. Technologies are interrelated. Yet, if one starts out with the idea that because they are interrelated you can't separate them, you can't do anything about assessing individual technologies, and that is a conclusion that doesn't get you very far. I think, rather, one has to start out with the idea that there is indeed some degree of separability, but one needs to keep in mind that individual technologies are interrelated.

In any kind of technology assessment there is a need to talk about definitions, directions, and magnitudes of the technology and then define specifically the kinds of effects one will gauge that technology against. In the case of our corn technology

Table 1.--Framework for technology assessment

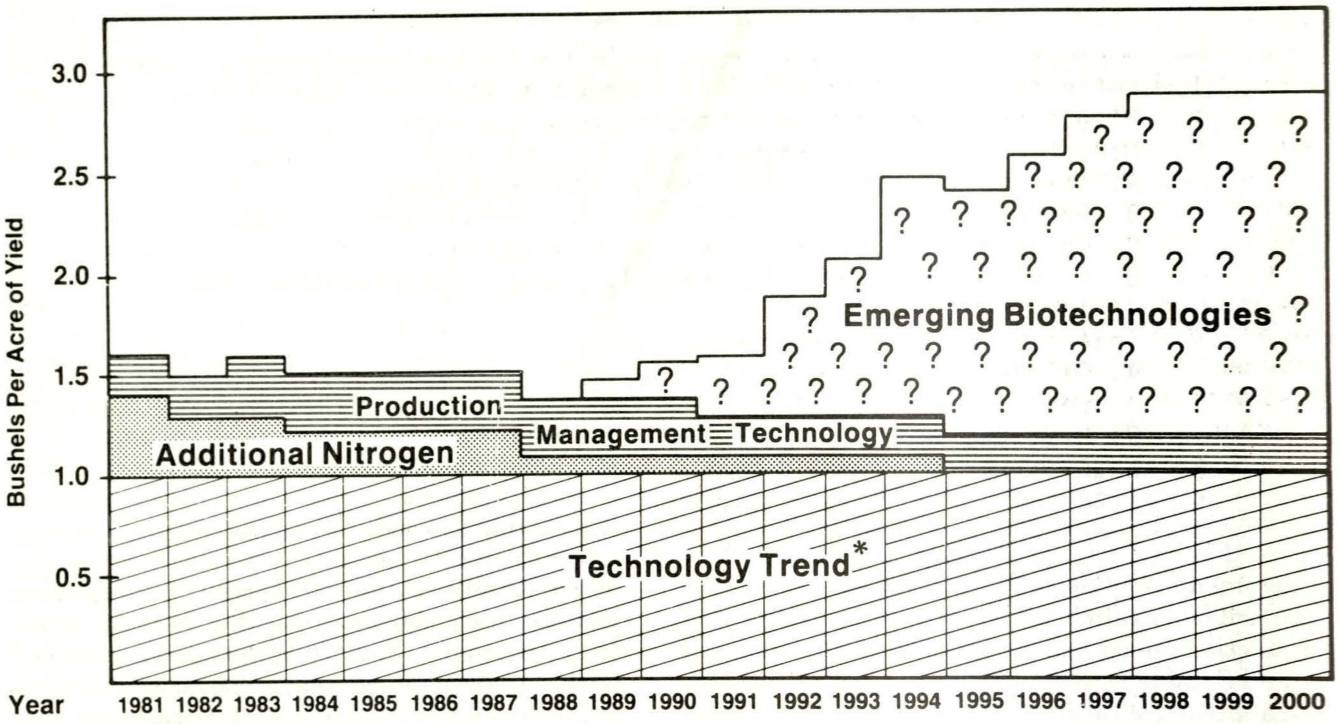
1. Definition and description of technology.
2. Direction and magnitude of technology.
3. Direct effects of technology on:
A. Yields, costs, profits, production capability.
B. Productivity.
C. Input demand.
D. Economic, environmental, legal, social, institutional, etc. considerations.
4. Indirect effects of technology on:
A. Gainers vs. losers.
B. Long term effects.
C. Risk and uncertainty.
5. Feasibility of technology.
6. Alternative technology options.

assessment, we centered strongly on a set of evaluative criteria dealing with yields, costs, profits, and production capacity, those listed under item three A, B, and C (table 1). When it came to section D, we took the approach of raising these questions while realizing that we were going to deal with them very inadequately in a limited amount of time and with a limited effort. This framework is a useful next step after one has talked about technology assessment in complete generality. This starts getting down to the topics and issues that you can think about, what kind of data there are to work with or which can be developed, and what kind of analytical framework can be used. Rather clearly, some other evaluative techniques, such as the rate of return or other kinds of analysis, would just start at a point in this framework and move to a rather specific end, whereas the technology assessment framework in general has a lot of flexibility. Even if one finds he can't do too much on the individual subtopics (in table 1), it is still useful to set them out, identify them, and ask the question of whether or not you can do something meaningful in an evaluative mode. I think that it makes some sense to raise questions about the feasibility of the technology and how it relates to the alternatives available. Looking at things in a tradeoff or alternative framework is a useful device in generating perspective, compared with just looking at the technology without some reference to the alternatives that are available.

Finally, in our technology assessment for corn we were really assessing historical technologies. Yet, I think one can use that historical analysis to sort out the important factors and project them into the future. One of the significant things that we concluded was that in the case of corn production, some of the old technologies during the 50's and 60's, such as nitrogen fertilizer, had dramatic impacts. Yet, as we moved into the analysis and measured the marginal product of an additional pound of nitrogen in the 70's, it was only about 20 percent of the marginal product of an additional pound of nitrogen in the 50's and 60's. For an obvious set of reasons, we were observing the law of diminishing returns in action. By the 70's almost all corn producers were using nitrogen and at very high levels. So I think one can get some feeling about the future from looking at past events. We did do some heuristic analysis of the emerging biotechnologies that might be applied to corn production. One of the conclusions was that, if we are going to get major future increases in productivity, and a yield gain beyond the fairly consistent increase of about a bushel a year that is associated with traditional breeding of improved varieties, it is going to depend heavily on this area of the emerging biotechnologies (figure 1).

Largely as a result of that, I and a couple of colleagues have two fairly significant efforts going at the moment trying to evaluate the time path and a potential payoff for investments in the emerging biotechnologies. I think anything we can do to uncover things in this area is going to be useful. Even if one looks at the role of the emerging biotechnologies in plant production, as compared with animal production, there are some very different recognizable benefits. The more immediate ones appear highly related to the livestock sector, whereas in the longer term, the major payoff might well be in applications to plant production.

Let me summarize a couple of the advantages of the technology assessment framework (table 2). It does permit the use of multiple criteria for evaluation. In this framework one can evaluate against a whole variety of criteria, from energy efficiency to cost efficiency, to general productivity, to externalities generated by the technology, etc. And it has a



* Includes conventional plant breeding and other highly correlated trend factors such as plant population and moisture control.

Figure 1.—*Projected marginal impacts on corn yields by various technologies, 1981-2000.*

good deal of flexibility. At least conceptually, one can consider both market and nonmarket criteria, whereas in rate of return analyses one is pretty much restricted to market criteria, and to shifts in supplies and prices. That doesn't mean that it is easy to come up with nonmarket criteria, and I think that is a problem we all face. Technology assessment does permit the evaluation of a whole production system, and the linkages within that system. It leaves wide open the question of appropriate analytical proce-

Table 2.--Characteristics of technology assessments

ADVANTAGES OF TECHNOLOGY ASSESSMENT FRAMEWORK
1. Permits multiple-criteria framework of evaluation.
2. Flexibility.
3. Can consider both "market" and "nonmarket" criteria.
4. Permits evaluation of the whole production system and its linkages.
5. Does not exclude any analytical procedures and uses information from any source.
DISADVANTAGES OF TECHNOLOGY ASSESSMENT FRAMEWORK
1. May not generate unique (specific) answers to questions under consideration.
2. Analytical framework is somewhat "ad hoc."
3. Framework does not, by itself, provide a "weighting basis" for multiple criteria.
4. Technologies are often not independent of each other.
5. Interaction of price and technology can alter the outcome.
6. There is a tradeoff between narrower, in-depth evaluation and a broader technology assessment type.

dures, and one can utilize information from any source. One of the handicaps of being locked into just a single analytical technique is that you can't find a way to effectively use some of the information that is available. One can focus on different dimensions and technologies, depending on what the assessment objectives are. In retrospect, I think one of the problems we had with the corn technology work was that we didn't narrow it down tightly enough as to what our assessment objectives were. We could have narrowed it down and made it more specifically an assessment of past and future research investments. It's important to have firmly in mind what your assessment objectives are.

Then, on the other side of the ledger in broad terms, one has some inherent disadvantages associated with technology assessment (table 2). First of all, it may not generate unique or specific answers to questions that are under consideration. You may not specify the objective functions that are going to answer the questions that you want. Some people would argue the analytical framework is likely to end up being somewhat *ad hoc* because there is no specific analytical technique that the technology assessment framework directs you to.

One can, and probably does, end up with an eclectic kind of analytical framework. The framework does not, by itself, provide any weighting basis for individual criteria when you have multiple criteria, such as productivity, energy efficiency, or something else.

One other problem is that technologies are often not independent of each other. There are a variety of interactions of other developments along with technology that can alter the impact of the technology. I would take a strong position that technology assessment is not a substitute for narrow, in-depth evaluations of technologies, because one is almost by definition faced with having to aggregate over some of the details of individual technologies. In fact, particularly with respect to the emerging biotechnologies, when we tried to get highly technical we started encountering conflict between individual scientists. There was a point beyond which, if we tried to get more detail, we were in fact trying get into the highly technical area of new technology which we couldn't do effectively for a couple of reasons.

First, we didn't have the intimate technical know-how that was needed. Second, when you get too near the very frontier of what is going on in research, then you find very capable scientists with very different feelings about things, and it is pretty hard as an economist to say who is right and who is wrong.

My final comment would be to suggest that the future of technology assessment has to be very bright, both from the standpoint of a mechanism to look at the question of evaluating investments in research and development, and also from the standpoint of being able to deal with multiple criteria. I think the time has come and gone when we could impose on society a judgment about whether a technology is good or bad, or desirable or undesirable, without looking at something more than just a rate of return or a simple productivity gain number. I think we must have a broader framework. That doesn't mean we must have it in each individual research evaluation activity, but somewhere along the line in making judgments about research investments, and technologies, we have to be able to look at more than just a single criterion of profitability, or productivity gain, or whatever. I would urge people in utilizing this kind of a framework to start out with the notion that evaluation can be pretty broad. Try to specify a broad framework, then work down to the more specific things that can be done within your set of resources, data, and analytical capabilities. I like that direction much better than starting out with an analytical technique and looking for a set of data to which it can be applied.

SUMMARY: REFLECTIONS ON FORESTRY RESEARCH EVALUATIONS

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Washington, DC

THE POLITICS OF SCIENCE AND BUDGETING

I would like to make a few observations on science in forestry. Much of my perspective deals with the politics of science and budgeting. Yesterday, John Fedkiw mentioned the introduction by Robert McNamara in the late 1960's of Program Planning and Budgeting (PPB) to the Federal government. John and I worked together in those days along with Clark Row, Bob Marty, and others. I accepted the PPB system with much enthusiasm; I thought here was a way to develop a rationale for science programs that would be useful. I ended the PPB era with a great deal of disenchantment. With the exception of the tree improvement work in loblolly pine, I found the PPB exercise demanding of information and extremely sensitive to assumptions. I had the feeling that the analysis often frustrated or thwarted research programs rather than enhancing them.

In the same vein, a high-level science administrator in the USDA observed that almost none of the changes we see in science policies in the Federal government were initiated by evaluations. For example, the space program wasn't the result of an evaluation, it was a response to the Russians' launch of Sputnik and its implications for national defense. A few years later President Kennedy said we were going to put a man on the moon in 10 years, a goal around which everyone, for a variety of reasons, could rally. Today space programs still claim about $5 billion of the research budget of the Federal government.

And human health programs have grown enormously in the last two or three decades, but not as a result of formal evaluations. Those advances came from the fact that Mary Lasker prowled the halls of Congress, lobbying for various health institutes in the Federal government. I think human health research is now over $5 billion per year.

My point is that evaluations play a relatively small role in the development of Federal science programs. However, as John Fedkiw said yesterday, in order to take advantage of opportunities that come along there needs to be a credibility, a legitimacy, and an understanding of science. And that's what leads us into the study and evaluation of science and the education process itself.

THE NEED FOR EVALUATIONS

Evaluation activities, as I said earlier, give a legitimacy and a credibility to research. We must all recognize that budgeting is still largely a political process. In presentations of budgets with the Chief, I serve as an advocate for my programs and he normally serves as an adversary. When the Chief presents the Forest Service budget to the Secretary of Agriculture, he tends to serve as an advocate for the agency budget and the Secretary and his staff play the role of adversaries. And I am sure that when the Secretary talks to the Office of Management and Budget, he serves as an advocate for the Department budget. So we have role reversals among participants, a process I consider to be entirely rational.

Publicly supported research is strongly conditioned by political and social concerns, whereas evaluation is not. It is the absence of this influence that makes evaluations useful. There are several areas where I think that we need help from evaluation in better justifying and allocating research effort.

Evaluations of Noncommodity Resources

The first is a need to address noncommodity items, which do not enter the marketplace and therefore do not have traditional economic value. Examples include recreation, urban forestry, wildlife, and water resources. The Congress, in the last few years, has shown some inclination to support those items in

National Forest Systems programs. The outdoor recreation management budget for the Forest Service alone is now in excess of $100 million, yet the research program that supports outdoor recreation management is in serious decline. Up to this point our best argument for the recreation research program is that it backstops a more than $100 million outdoor recreation management program in National Forest Systems, and also provides valuable input to the outdoor recreation management programs of the Corps of Engineers, the Bureau of Land Management, the National Park Service, and the Fish and Wildlife Service. Somewhat over a billion dollars of Federal funds are spent each year on outdoor recreation management programs, and, with the exception of the Forest Service, there are no research programs in any of the management agencies mentioned above. We need evaluations to further analyze noncommodity research programs.

Evaluations of Sustaining Research

A second area where evaluation can help justify and enhance understanding is in maintenance or sustaining research. As the years go by, I have become more and more sensitive to this issue, but it is still difficult to articulate it well. We tend to describe research programs in terms of emerging issues and new problems. In the last 2 or 3 years, whenever one of these issues has surfaced, the policy response has been, "That's a good idea, let's do it, but within the budget you already have." That has been the reaction to research in acid rain, old growth-dependent wildlife, and others. But I have come to realize that a majority of our program is sustaining and continuing research, without which productivity levels would decline. These maintenance research programs do not result in great gains in the availability of a resource; rather, they allow us to maintain what we currently have. Maintenance research is terribly important to the well-being of the forestry economic sector, and I suspect this is true in agriculture as well.

An example of maintenance or sustaining research is prescribed burning. This is a growing technology and one that ought to be used more on forest land nationwide. However, the risks for those who use fire are high, as the reward system doesn't recognize the successful use of fire nearly as well as it administers sanctions for the misuse of fire. I am reasonably sure that if we were to drop that sustaining or base research program in fire, we would see a decline in the use of prescribed burning. It's important to have a research presence, to feed new technology into prescribed burning, and have a source of information and support for those people who are administering fires under risky conditions.

Another area where sustaining research is important is in reforestation and regeneration. As reforestation research declines and we go through one or two generations of new district rangers and new foresters, we begin to see a decline in the quality of regeneration programs. Reforestation specialists continue to depend on the most up-to-date information on regeneration techniques, brush control methods, and site preparation methods to perform their jobs well.

Role of Education in Science

A third area where I think we need evaluation assistance is in the link between education and technological innovation. We need to study the relationship between formal and informal education on one hand and research on the other. My impression is that our extension activities in forestry are much less than they are in agriculture. It's an area that needs additional examination, and I hope we can join with the Cooperative Extension Service, State foresters, and State and Private Forestry to take a better look at that linkage. Education, both formal and informal, always gets high marks as a contributor to technological change.

Historical Perspective

A fourth area in research evaluation studies the evolution of science policy and management in forestry over time. In my estimation, one of the least understood stories in conservation is the resurgence of the forest resources in the South. It is the premier story in forest conservation in the world, exceeding the eucalyptus story, the poplar story, the *Pinus radiata* story, and perhaps all of them combined. Yet it is a story that is not well told. In the early days in the South, a relatively unsophisticated technology required getting livestock off the range, hogs out of the woods, and particularly stopping fires. Education played a major role in these changes, along with some important legislation. Fifty years ago much of the South was worn-out cotton fields and broom sedge; very soon the South will provide 50 percent of the forest products harvest in this Nation. It is substantial disservice not to understand what science and education contributed to this remarkable story.

I want to reemphasize again the point that we must keep in mind the historical contribution of science and education. We had one Regional Forester who not so long ago said, "You know, I can't think of anything that research has ever done for me." I would like to have placed some well chosen case histories in front of him.

Secondary Consequences

My fifth point, which has been addressed by others, concerns the second- and third-order consequences of science. These consequences, including income redistribution and social and environmental impacts, are examined in technology assessments. A favorite question of former Forest Service Chief John McGuire was, "What do our policies have to do with industry concentration, or the lack of it?" There are a multitude of these issues that we must begin to explore, but I realize that it is far easier to frame questions than it is to deliver answers. The set of second- and third-order issues that Sundquist and others have described in their technology assessment of maize illustrates a pattern that we could well adopt in forestry.

Science as a Policy Issue

My last point has to do with the consequences of science as a policy issue, as a factor of productivity in its own right. One of the most memorable sessions that I attended in my years in Washington was the prime agricultural land seminar sponsored by Resources for the Future. The genesis of that study had to do with the consequences of increasing urbanization, transportation, and utility corridors on prime agricultural land in the U.S. The discussion quickly developed into the role of technological change in agriculture. Emery Castle made the point that about 2 percent of our lands are going into urbanization, and there is some indication that that may be running full course. He also indicated that an additional 2 percent of the land is going into transportation and utility corridors, and that too may be running full course. But despite these declines, over the past 50 years, innovations from science have probably increased agricultural production by approximately 50 percent. Emery went on to say that perhaps we turn our attention to science policies because they will make more difference than all of the land withdrawals combined. I suspect that a similar pattern exists in forestry.

An example is the demand-dampening implications of truss-frame housing. If we had a 100 percent implementation of new truss-frame housing concept, the savings in timber volume would offset the timber supply consequences of all the RARE II areas on National Forests in the country. I am reasonably sure that truss-frame housing will never enjoy 100 percent implementation. However, this relatively low-cost technology has large implications for taking pressure off timber productions, thus liberating forest lands for other uses.

When I was here at the University of Minnesota 35 years ago the big issue was the advancing age of the logging labor force. What could we do about it? As it turned out, it was a nonissue, because we substituted capital for labor. One of the things that sticks in my mind is that in 1950, a 25-man camp in Koochiching County could put up 100 cords of wood in a week. When I was in northern Minnesota about 3 or 4 years ago, I saw a five-man crew put up the equivalent of 150 cords of wood in 1 day, all because of capital investments in modern machines—which in turn owed their existence to research and development.

There are policy implications in that logging example, and other examples like it that have arisen out of technological change, that we've not pursued. For example, I've always wondered why the environmentalists, who are seeking additional Federal lands for nontimber use, have never seized on the opportunity to substitute technology for land. If we would apply some of the technology currently available to forest lands that are potentially more productive, and environmentally less sensitive, we could have both timber and outdoor recreation. The larger issue here is that science and technology can be substituted for other factors of production such as land, labor, and capital. The role of science in substituting for the other factors of productivity deserves more attention in forestry. John Fedkiw pointed out the enormous gains in productivity in agriculture because of science. I suspect those gains also have taken place in forestry, but we simply haven't documented them very well.

DISCUSSION V

JOHN FEDKIW

You have to make some sort of decisions on technology, or where technology is going, in this whole area of technology assessment. Important questions you must deal with are what new technologies are coming, and what their quantitative impacts will be. I am curious as to how you go about making those decisions and getting consensus or can somehow be confident that yield is going to go up a certain amount, or the acreage required to produce a given amount is going to go down. These are very important pieces of information.

BURT SUNDQUIST

At some juncture one goes beyond the point at which systematic inquiry or research can add to the total information package. There is no question that a wide variety of factors enter in. One can start out with the idea of trying to assess what's happened in the recent past and then try to see what kind of factors are likely to constrain or augment that area of technology in the future. I don't think there is any simple answer to that question. I think that the more one can disaggregate and look inside the "black box," the better off one is, because it's very dangerous just to extend historical trends without taking a look at what kind of things are likely to impact on them.

JOHN FEDKIW

When you end up with a number, is it your decision or do you take the consensus of scientists working in this area? Is it your own, private assessment, based on inputs from other people, or is it a consensus decision by the key people in this area of work?

BURT SUNDQUIST

It's a combination of several things—a combination of the appraisal of so-called experts or scientists with respect to some areas, such as the emerging biotechnologies. With respect to the impact of nitrogen fertilizer, it flows out of the economic analysis based largely on what's been happening over the last 20 or 30 years and extending that. It seems to me that any reasonable appraisal of the future with respect to production, output, or yields has to use information from a combination of sources. In some areas, you have a good historical base to use a technique such as regression analysis. In the emerging biotechnologies, you don't have an historic database to work with, so it seems necessary to look to people who appear to be the best informed. In some cases, we talked to people in an effort to understand the importance of a given technology, how it operates, where it came from, and what the likely future of it is. For example, several people that we talked to for the assessment of corn technologies were breeders, several others had worked intensively with such things as corn-drying technology, or irrigation technology. I would say with the exception of the emerging biotechnologies we didn't lean too much on these people to give us estimates of future impacts, or even partial impacts. We had them help us understand the technology of what brought the changes about, and how it related to other technologies.

DAVID LEWIS

Along the same line, given the 50 or so people who were involved in your assessment, would you get general consensus about the gains in corn production that we are going to get from the application of nitrogen, with a fall-off to zero in a certain time, and the same thing for management? The implication of all this is that we ought to concentrate the resources for corn research at least in biotechnology, because that's where the big payoff is. Can you get a consensus from the people in the corn program who would say that makes sense and that is the future direction?

BURT SUNDQUIST

I think we had a reasonable consensus; I tried out the preliminary results on people, and by and large they didn't find any substantial issues. When you look at those projections a little more closely, you see that there is something called a technology trend. But as a practical matter, we can spell that out in quite a bit of detail, and say that 95 percent of it is tied up in traditional plant breeding improvements, moisture control, etc.—but the biggest single item is plant breeding. There is also a significant improvement due to management and improved information and control systems—doing a better job of applying the technology that is currently available. Feeding into this are things like more systematic scouting with respect to disease and insect problems, and better control of irrigation technology, which is a significant variable. But I think our assessment of fertilizer technology is that it has made its major contribution.

That past contribution will continue to be there, but when you look at further increases in yields, well, we ran into a number of people saying that they're advising top corn farmers to cut back a little on fertilization. At the margin they're spending a dollar and maybe only getting a dollar back, and there are better places to spend their money.

ANNE FEGE

Dr. Buckman, most of your comments were directed toward lines of research, that is, subject areas we should look at in forestry. There is a whole other part of research evaluation that deals with how we can manage the existing lines of research better. My question is, what do you see as the most important questions from the standpoint of research managers, and how did those questions stack up against the list that you provided us?

ROBERT BUCKMAN

I think a fair amount of work has been done on the characteristics of researchers. When one gets to the smaller units of work—and publications and things of that sort are useful, provided you know something about the environment and the characteristics of the person you are dealing with—I am just terribly concerned that the error terms that go with some of the these correlations may be so large that the opportunity for some injustice is considerable. I think the most powerful tool we have for evaluating research is a peer review system. So anyway I don't mean to discourage you from using some of the techniques like citation indices, and numbers of publications or whatever, but I am still concerned about the size of the error terms that go with those.

ANNE FEGE

Is it important to reduce those by looking at whether some of these tools are some that we can use as research managers? I guess that's a more academic line of work to be worked on later, at least to take a look at whether it may or may not give some answers to management.

BILL HYDE

It seems to me there is a second-order effect you want to think of, looking at judging research by locations. I recently heard a Forest Service pioneer researcher speak and I am absolutely certain that he was serious. He commented that if it is publications that we want he will produce publications, and we all want to be very careful in this area. That's certainly not the intent of anybody here. But that can become one of the outcomes of some of our work.

ROBERT BUCKMAN

I am concerned that we will get goal displacement at the level I work at, if we put the emphasis on publications. Now I think one of the stations uses publications as a diagnostic tool, but they use it on almost a case-by-case basis. I think it is a powerful tool, but it requires a lot of judgment to go with it. Some of the questions we are called upon frequently in the budget process to answer are, how do you evaluate your research program? What kind of turnover do you have? How do you address new lines of work? So we do have to have a way of addressing those issues.

GARY EVANS

One of the indices that we use in CSRS, especially in the McIntire-Stennis program, is one similar to that question. We use it strictly as an index. We look within an institution at a number of graduate students, and number of publications per scientist, and we break the number of publications out into refereed and nonrefereed. It is just one of the many kinds of indices of research productivity that are available and can be used. And it helps us, as we look at 62 forestry units, across the country, involved in research, to maintain an index of which ones are coming up and which ones are dropping back. But again, the error term is so great that, all we can use, is a single-integer index.

ROBERT BUCKMAN

One of the areas of inquiry that I often thought could be useful, provided we can get around the Privacy Act, and the Freedom of Information Act, is the panel evaluation itself. The panel evaluations reflect a number of things, including a peer assessment, about how well the scientist is doing. I have often wondered if there are any opportunities to correlate the consequences of the peer review system with other characteristics of those scientists.

ANNE FEGE

You certainly have the promotion records, and we know how many years they have been in grade, and each time they go to panel. From an internal standpoint you may not be able to do it as an academic exercise, but that information is certainly available to station managers.

DENVER BURNS

Certainly information on publications is an indicator of a person's capability in many areas. I know some of the early work done at the North Central Station in the CRUSK study showed that the same people who published a lot are the same ones who perfected technology transfer, are called on for seminars, and receive recognition in other ways. All of those go together and relate to the figure stated earlier, that 47 percent of the publications are written by 14 percent of the scientists.

JOHN FEDKIW

As a final note, I refuse to work with policy questions and take those policy answers and build them into the assessment process without direct involvement of the policy client. The policy process is a way to involve the interest of policy officials, and provide them the opportunity to respond to new or improved information. Unless you have a policy official's interest in an issue, you may just not get much response. The strategy of providing information for policy issues is worth thinking about real hard.

ROBERT BUCKMAN

I'm hoping that a lot of this policy initiative, and the larger policy issues, will emerge from some of the studies I've heard about at this workshop. That's where they ought to arise. We tend to focus on the technologies and not so much on the policy questions stemming from them.

LYNN MAISH

It's clear that you're pretty much covering most of the waterfront in these studies, including those that might provide information useful for research management in the narrow sense, such as outputs in terms of publications. That kind of study is of very limited policy interest at the Department level, except that it is of interest to know that efforts are being made to look for ways to improve management. The fact that you're doing things like that is the main point for a USDA policy analyst. There are some other studies that seem to be providing some information for management purposes. A more in-depth look at how a project gets initiated, how you organize efforts to conduct the research, and so on would be useful. Dr. Buckman mentioned in his areas of interest some work with the interaction between extension and research, and facilitating the transfer of technology. That ties in with the question of how you choose a research project, design it, conduct it, and transfer the results.

The other day I was in a briefing on a very interesting study done in agricultural research. The USDA picked a number of successful research and technology transfers produced by ARS. They did a little case study on each one, and they had talked to the scientists on the project, and talked to user groups. The evaluators classified each of these cases by the extent to which user groups had been involved in the definition of the problem and design of the project, and by the extent to which there had been interaction between scientists and users during the conduct of research. Finally, they classed them by the nature of the dissemination of results—publication or presentation of the papers at a conference, active communication by the scientists to the users, or by the intermediary of extension. They classified all of the cases by those conditions, and then they talked to the users to get information as to which had been successfully adopted. The evaluators were able to draw a number of interesting conclusions from that kind of study. For example, in those where the dissemination of results had been very passive, a very low percentage were successfully adopted. The highest percentages of successful adoption were among those cases where they had high levels of interaction at all three points. That's an illustration of a study that might be useful in providing information in the organization and management of a research process.

This also gets to the point that we don't get the payoff of the research until the results are adopted. I noticed that we do have some studies on diffusion of results. I'm not sure whether any of these are in this broader context that goes all the way back to the interaction with the users and the actual initiative of the research, or interaction with extension. A number of studies are looking at rates of return, some in the narrow sense and some in the broader aggregate. Once you've established the credible, approximate levels of rates of return, then a lot of issues arise for which you need information beyond rates of return. This means that impact assessment or impact analysis must be done. It gets into questions of distribution of income, and other effects. Clearly you haven't gone very far in this sense, but it is of policy interest.

HANS GREGERSEN

Where do you see the agricultural research evaluation work going? Is it going towards more technology assessments, or will there be a broadening out of the rate of return studies? A lot of people seem to have decided that we have done enough of those.

BURT SUNDQUIST

I honestly believe that we have about enough of those, with the exception of trying to update them

through time and keep them current. Further evaluations probably ought to be more specific, or more functional area-specific. For example, even looking at corn technology, I would judge there's a lot of difference between additional mechanization research compared with biotechnology research, or plant breeding, or fertilization research. I think we have used up a lot of the potential with respect to mechanization, which has already driven the labor inputs in corn production down to less than 5 percent of the total costs. Moreover, we have only adopted part of the mechanization technology that's already available. If one compares further mechanization research with some other area of technology, or functional research such as plant reproduction, one ought to conclude that we should put additional mechanization technology lower down on the list of priorities than some other things. I think that kind of insight is very useful and you don't get it by doing aggregate rate of return studies. The aggregate rate of return can be very high and yet you can have a very substantial, very redundant research components in the system. So I think you have to try to get as much specificity as you can, whenever you can get it.

HANS GREGERSEN

So you actually see more disaggregation than in forestry research evaluation. We've been talking about a higher level of aggregation. You're saying you see some disaggregation, getting more specific research program evaluations.

BURT SUNDQUIST

It depends on where you start from. Looking at Federal livestock research in agriculture, I think that if we lump together the dairy sector, the cattle feeding sector, and the swine sector, we are aggregating over tremendous gains in some of those areas, but in some of them we've made very little gain. Poultry is very different from the rest. So to generalize from aggregate rates-of-return may generate as much misinformation as information.

So, keeping in mind where you can break out data and where there are definable lines of research, to the extent that you can trace those avenues, that would make more sense than doing a lot of additional rate of returns.

JOHN FEDKIW

The point here though is that this is ex ante, *which by definition isn't very useful if it's too aggregate, as opposed to* ex post. *That's a subtlety that's important. The National Academy of Sciences is developing some systems analysis methods to look at multiple variable behavior over time in response to different types of technology. They are trying to get at some of the subtleties of new technology, the interactions and side effects, which point in the direction of more complete and more effective technology assessment work.*

DAVID BENGSTON

One of the things we found in reviewing and assessing the agricultural research evaluation literature is that the number of empirical studies is beginning to taper off. Plotted over time, you get an "S" shaped curve which is on the decline. So perhaps there is a change in focus away from the rate of return studies and toward the broader technology assessments.

ALLEN LUNDGREN

This is interesting from the standpoint of developing a strategy for research. Of the inputs into corn production, the price of the labor component was only 5 percent. This means that at most, if you almost completely eliminate research on that component, you would only affect 5 percent of the total cost, in terms of cost reduction. Even if you were successful in further reducing it, you don't really gain much. Have you done this kind of analysis as a strategy for looking for opportunities to have major impacts on crop production systems? Have you explored that further—in other words looking for those places where, if you could have an effect, you would have a big impact?

BURT SUNDQUIST

Not in a major way except to conclude that we have pretty well mechanized our agricultural operation. As you proceed to utilize the major agricultural land resources which are available rather fully, this pushes you in the direction of assuming that any major future gain productivity will have to come out of increased output per unit of land. In other words, yield-increasing technology. It seems to me that this gives you some insight into the need to sort out those technologies that have yield-enhancing capability from those that don't. In some cases, it's difficult to sort out how much yield-enhancing capability a new technology of this or that type might have. But some technology appears to have very little yield-enhancing potential. And, in terms of the involvement of our technology and science generally, it's hard to put things at the top of the list unless they have at least some yield-enhancing capabilities.

JOHN FEDKIW

Of those seven items that Bob Buckman cited, the first three, as I understand it—the maintenance re-

search, noncommodity items, and the mitigating research—don't sell well. They don't sell, and I presume the reason for it is that people cannot visualize the benefits. But if you have something that is going to reduce costs and increase yields, they can see that and it may be adopted. Impact analysis is needed for some of the other areas. The role of research in influencing total productivity is understood. How much research has contributed to that is the quantification question. The second- and third-order consequences are also a part of quantified evaluation. When you're talking about the consequences of science on policy and productivity issues, productivity arguments do take you any place in a policy area until you have some numbers to talk about.

However, there comes a time when you have done enough program evaluation, ex post *perhaps, when more evaluation may not have a very high priority. After you have done enough* ex ante *studies, you perhaps learn how to be more selective about where you do it. We're still in a learning mode and the developmental stages of information analysis.*

GARY EVANS

One of the things I would be interested in hearing about from the group is the development of methodologies for evaluations on forestry research. What are some of the major frustrations? Where are the big holes? Several people have indicated that they're just now becoming able to define some kinds of quantitative evaluation techniques. In relation to the studies that we have in front of us, how did most of the people doing these studies feel about where the methodologies are? I think this would be of value to us as we look at supporting future evaluation projects.

HANS GREGERSEN

The real problem we face when we talk about methodology is fitting it to what information or data you have available. It's the old idea that in an economic development area people say that the economist isn't doing a decent job. It's because you don't have the production function data, you don't know what the physical input/output relationships are to put those values on. The economic part of it, I think, is fairly simple in most of those cases. It's really the relationships between physical inputs and outputs that we don't have and that's where I see the real challenges and problems—getting the kinds of physical input/output information that is needed. In agriculture, they've built up quite a solid base of information. If you're talking analytical techniques, I don't think that's really where the key issue is. I think we've got plenty of those around. Are you talking about methodology in a broad sense of data generation, too?

BILL HYDE

I think the data problem is overblown. Like I said yesterday, we can't let this hold us back, otherwise we would give up and clearly this isn't a time for giving up. Yes, the data problem is a very severe one. We have problems, for example, in estimating the benefits. We're collecting benefits industry-wide and the costs we're looking at are government costs. Clearly there are some private research costs, too. It's my understanding that there is an attempt by the National Forest Products Association (NFPA) and the Forest Service to develop better estimates of private costs. Meanwhile, we have to use all sorts of half-good methods to come up with industry costs or proxies for them. Furthermore, we have to start looking at biological research. Biological research investments create additional benefit estimation problems because of the long time periods involved. We are looking at production functions that are at an absolute minimum 25 years for any tree species. This means that we also need comparable data for 25 years ago and for today. We probably don't have it.

Among other things, such as the spillover issues, how much is forestry really gaining from the Finnish sawmill experts, or in a more extreme case, from the steel industry where we are getting better sawblades? What is the impact of the chemical industry on pulp and paper? We are just beginning to figure these things out. And what about development and retooling costs. It is awfully easy to overlook these altogether and, thereby, overestimate net research benefits.

DAVID BENGSTON

I have one comment on holes in the methodology. When I look at the studies that have been done, the agricultural studies and others, one thing that I see is a lot of very naive assumptions about the future diffusion of innovations. Often, extremely simple assumptions are made to handle that. There's a wealth of information and a large array of techniques in technological forecasting that would be useful to research evaluators. Stier has done some work in this area on diffusion of the sulfate pulping process. That's one of the few studies that has tried to forecast the diffusion of a major innovation in forest products. There are techniques available that could make research evaluations more credible that just aren't being used. I think the problem has been that after you've gone through all the agonies and trials of trying to quantify research benefits and costs, you don't want to spend another year developing a sophisticated diffusion model to get incrementally better information.

ROBERT WESTGATE

Something that I've noticed in my containerized seedling study is the paucity of data available today. I had to estimate 11 variables in my study and there were only data on two of them readily available; the rest had to be estimated through various techniques which often leave a lot to be desired.

We have been talking about the diffusion of research results, or the diffusion of technology. I talked to a person in State and Private Forestry in Utah about prices of bare-root seedlings as compared with container seedlings. He told me that he didn't recommend container seedlings to people that he had advised, because they were about $4 per thousand more. He just didn't think that they should have to pay that much more for container seedlings. There is a lot more to reforestation than the price of seedlings. Now when you have people who don't even know what the price for seedlings really is or should be, you get into a lot of problems. I don't know how hard it is to get this kind of data or how much it would cost, but it just strikes me that before we worry about forecasting and things like that, we should worry about what's available today and what isn't.

ALLEN LUNDGREN

One of the areas where we seem to be most lacking in information is records of adoption of various kinds of practices, even records of activities carried out, such as the number of acres cut. These things are not readily available. You could get them but it's likely to be a costly operation. Sometimes the budget for research simply won't allow you to go out and collect all the information you need and you could spend the whole research budget on doing that alone. This is a real problem we face. In agriculture, there is an enormous data-gathering network that's producing information about the rates of adoption of corn varieties and other detailed data. For example, the number of acres planted, even down to the county level, and things like this. Those records are summarized and available but that isn't true in forestry.

BRUCE BARE

In calculating the rate of return study on research investments, when do you count the benefits? When research is adopted, or when the results of the adoption themselves are received? For example, if your research increases yields, but you don't harvest trees for 30 years, when do you count those benefits?

ROBERT WESTGATE

You count them pretty much from when they start. For example, I started counting container benefits when containers were actually being planted in a reforestation sense. Not in a research sense, trial planting and such, but actually being planted by industry or whoever plants them. In this case, it started in 1970. I consider that adoption, when it is actually being used.

BILL HYDE

You counted that because at that moment you got a change in cost. If you were addressing yield increases, you would count them 30 or 40 years later, whenever they occur.

HANS GREGERSEN

That's why, as we were saying before, almost by definition, it's impossible to get a very high rate of return. Can you imagine a 240 percent rate of return when you're getting benefits 30 years down the line?

BRUCE BARE

Then you don't end up with a return to research in that case. Instead, you end up with a joint rate of return on the research investment plus the cost of adoption. How do we interpret this?

HANS GREGERSEN

What the ag economists have faced is that there are different adoption costs and quite often they simply made assumptions about those costs. The common figure is 30 percent of research costs, or something similar.

JOHN FEDKIW

The adoption costs may not be fully chargable, because the cost of producing may be less. It's the incremental cost that must be counted. Perhaps the production may be cheaper per unit. Then the adoption is really a net saving, except for those things that occur which stop production for awhile, or lead to obsolescence of equipment before the full value has been realized from it.

BILL HYDE

A power backup roller is a wholly new item, so its whole cost is incremental. The seed that is put in the ground for a new seedling may just be a cost change.

SUMMARY

WORKSHOP SUMMARY

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The formal evaluation of forestry research dates back at least to the Bailey and Spoehr report of 1929. Several reviews of forestry research needs have been made since, including the CRUSK report (Barnowe 1973), the report of the National Task Force of Basic Research in Forestry and Renewable Resources (1983), and the 1980-1990 National Program of Research for Forests and Associated Rangelands (USDA Forest Service 1982). Criteria for decision-making in research programs in forestry have also been described (USDA Forest Service 1981). However, evaluation in the sense of identifying the impacts of research-generated knowledge on the economy and society is much more recent. In agriculture, the Griliches study (1957) of the impacts of hybrid corn is generally recognized as the beginning of research evaluation in that field. In forestry, the Robinson study (1975) of technological change in the forest industries may be considered the beginning of research evaluation.

The boundaries of forestry research evaluation as a subfield of both forestry and economics (perhaps some would argue that evaluation is itself a field) have yet to be fully defined. It is an extremely young scientific endeavor, and the studies, both complete and just beginning, reported in this proceedings represent the majority of work done to date. Hence, these studies form the first outline, however hazy, of the type of information and methods that will be forthcoming.

As Allen Lundgren points out in his paper, the subfield of forestry research evaluation has yet to establish any traditions, scientific or otherwise. I hope and believe that this workshop represents the beginnings of such traditions, for it is through building on such bases that any scientific endeavor develops.

Research is now underway, or studies have been completed, for the evaluation of the returns to research in (1) forest products utilization, (2) forest harvesting, (3) forest fertilization, (4) timber management, (5) growth and yield modeling, (6) tree improvement, (7) reseacher effectiveness and productivity, (8) recreation, and (9) the role of research in industrial management decisions. Also, the first technology assessment of loblolly pine is under consideration, as is an evaluation of the use of forest survey information. Other studies not mentioned here are also in various stages of planning and consideration. These all are an indication of the vigor of this area of research.

Because this area of research is young, it offers many opportunities to extend our knowledge. Although several excellent studies have been completed, the base of *ex post* studies should continue to be expanded. While the results of *ex post* studies reveal the payoffs from past research rather than indicate the areas where future efforts should necessarily be made, they form an important base of knowledge for research management, and provide clues to the successful development of evaluation methods.

However, it is also important to develop *ex ante* methods of research evaluation. In this field of research, it is not always possible, nor often proper, to simply extend trends to project where research will generate the greatest desired impacts. *Ex post* methods are not always suitable, and *ex ante* methods must be developed, adapted, or adopted. The successful application of such new methods to alternative research programs will be most useful to research managers.

Forestry research evaluation appears to be following two lines of development. First, some studies are being done in the mold of those conducted by agricultural economists who have estimated the impacts of research on agricultural commodities. For forestry, this means evaluation of forest products utilization research. Timber management research also falls into this category, although no studies are yet complete. For beginning efforts in forestry research evaluation, this was the logical place to start. However, the nonmarket aspects of agricultural research have remained virtually untouched by evaluations. In forestry, a much larger portion of the research effort concerns nonmarket-valued outputs, such as recreation, watersheds, and wildlife. To claim to have examined all or even most of the research effort in forestry, these lines of scientific inquiry must also be evaluated. Hence, the second line of development in

forestry research evaluation is typified by the work at the College of Forestry at the University of Minnesota, which seeks to develop credible methods for evaluating recreation research. The knowledge gained can be applied to developing methods for evaluation of other lines of forestry research which have their impact primarily on nonmarket-valued outputs, such as water and wildlife.

An exciting development in research evaluation is the technology assessment pioneered by Burt Sundquist and his assistants, and discussed by him in this proceedings. This work develops evaluation to perhaps its broadest form, and its usefulness has received wide acclaim. An effort to apply this type of evaluation is now underway at Duke University.

The evaluation of forestry research is the study of the research process, from the work of individual scientists and projects and laboratories, through the dissemination process, to the identification and valuation of its impacts on society and the economy. As such, its results are of interest to a wide range of decisionmakers, including the doers of research, the funders, the disseminators, the administrators, and the users. This workshop represents a solid beginning of this promising and exciting line of "research on research."

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